THURSDAY, JULY 26, 1877

A MANCHESTER UNIVERSITY

THE movement of the authorities of Owens College, represented in their memorial to the Duke of Richmond on Friday last, is certainly one of the healthiest important educational advances of recent years. It is an outcome of an impatience of pure examination and a revived belief in the educative influences of association of young men with each other and with masters of the subjects in which they happen to be interested, which have been gradually but surely growing up among educationalists.

Like a few hundreds of other institutions, including Oxford, Cambridge, and the Scotch Universities, Owens College is affiliated to the University of London. Twenty years ago this meant that only students of these institutions could present themselves for London degrees. But their members multiplied—and every weaker member added to the list of affiliated colleges supplied a new reason for still farther widening the bounds of the University. It was natural that in time the outsiders educated at home, with private tutors, or in unrecognised and unaffiliated institutions should knock at the doors of the university and demand admission. After they did, it was found in the end impossible to refuse them. Since that date the University examines everybody, wherever he has been educated, and its influence is to assimilate what one may call organised institutions of individuality and character to the unorganised or semi-organised types of the crammer's school or the private student of cram books. For the school or the private student it has been and still is of the utmost value. For higher colleges it is a great centralisation, objectionable only because it is not complete, not having really swallowed up the Scotch and Irish universities as well as Oxford and Cambridge. Mr. Lowe's ideal doubtless is that the most intelligent, impartial, perhaps bloodless of examiners, selected from all the world, should prescribe the subjects of examination, and should thereby dictate from the standpoint of the highest human omniscience, the courses of all teaching institutions, and should, like Minos, Æacus, and Rhadamanthus, decide in final award the character and place in the universe alike of teacher and of taught. This ideal has been realised in the Chinese empire, and many excellent arguments can be stated in its favour. It is inadequately realised in this country, because all incorporated universities have been left practically outside of the scheme. Owens College is the first of the non-incorporated actual or possible universities which asks also to be released.

It is possible to offer the petitioners something they do not seek. Owens and other residence colleges object to be controlled by a mere examining board. But there are Universities like Oxford and Cambridge which are not mere examining boards, which have resident students, where the examiners are in frequent and living contact with the students, from whom, indeed, they are often removed only by a few additional years. Five years before, the men who preside over the triposes may have them selves sat for examination. Might not Oxford or Cambridge supply a less narrow and technical examination than London? Could these universities not affiliate Owens College to themselves in some more living bond than has now become possible elsewhere?

There is an excellent and all but final answer. Oxford was asked to do it for King's College, London, and after mature deliberation it deliberately declined. Probably it could not see its way to work out this more living association. There is one kind practicable —such as Oxford and Cambridge practise between themselves. A man may count a certain number of terms at Cambridge for his degree at Oxford, just as he may count so many sessions at Edinburgh for his degree at Glasgow. There is another kind conceivable. The teachers of both places may be associated with each other, with or without outside examiners, for the examination of the students of both, and the programmes of examination, and to some extent of teaching, may thus be settled in common. Oxford was not willing to associate itself with King's College in either of these ways, and there is no reason to believe that it would care so to associate itself with Owens College or any other institution. Such an association would imply an equality which the older universities can scarcely be asked to admit; and the second mode of association would institute a sort of outside interference with them which they would never allow. The simplest and most satisfactory university is self-contained, teaching and examining its own men under the stimulus of rivalry and public criticism, and with the help, perhaps, of outside examiners. Oxford and Cambridge see no reason why they should descend from their own secure and satisfactory position and tempt the dangers of confederation.

It remains to ask what good it can and what harm it might do to grant the prayer of the petitioners. The first question need scarcely be answered. It is an obvious advantage to every district to have a great centre of high thinking and noble living planted in its midst. But there is an equally obvious disadvantage in the undue multiplication of universities. There may be serious objections, as Prof. Huxley puts it, to any official system of branding our young herrings with B.A. or M.A. But so long as we keep to the system of branding we ought not to be too free with our brands. There ought not to be too many, and there are a good many in the United Kingdom with no means of telling which is a good brand and which is a bad one. There is no doubt that, as far as it goes, the objection is sound, and that we ought not to have too many degree-granting bodies. The question is whether we have—whether the precise limit at which we ought to stop has been reached—whether the new claimant is not as fully entitled as some of the old-established institutions to an independent status and existence. In England we have very few branding bodies, and every guarantee is offered by the Manchester authorities that their brand will be of the very first quality. They propose to have outside assessors to help them to see to it; the one thing they ask is that having the responsibility of teaching they shall have an equal share in directing the examinations. It seems a reasonable and modest request.

THE KINETIC THEORY OF GASES

A Treatise on the Kinetic Theory of Gases. By Henry William Watson, M.A., formerly Fellow of Trinity College, Cambridge. (Oxford : Clarendon Press, 1876.)

THIS book does not profess to treat of all that has been written about the kinetic theory of gases. It discusses the ultimate average condition of a material system, consisting of a very great number of parts in motion within a confined space, and it follows for the most part the methods of investigation given by Boltzmann. The discussion is arranged in the form of thirteen propositions, in which the different cases are considered in the order of their complexity. In the earlier propositions the moving bodies are supposed to be rigid-elastic spheres acting on each other only by impact, afterwards external forces are introduced, and finally the bodies are supposed to be material systems, the parts of which are held together by any system of forces consistent with the principle of the conservation of energy.

The ultimate average condition of such a system is investigated in a very satisfactory manner in this book. No part of mathematical science requires more careful handling than that which treats of probabilities and averages. Mathematicians, whose competence to deal with other questions is undoubted, have fallen into errors in treating of probabilities, and even the validity of certain methods of proof is still apparently an open question.

Besides this, some of the consequences to which these theorems lead us are so startling that we are not prepared to admit them without an unanswerable proof, and of the investigations already given, some are so short and incomplete, and others so long and roundabout, that it requires no ordinary exercise both of penetration and of patience to find out whether they are proofs at all. Mr. Watson has conferred a great benefit on the students of the kinetic theory by placing before them in a series of distinct propositions, none of them too long for the mind to grasp, all the necessary steps leading to the result, and none of the superfluous evolutions in which the mental energy of the student is so often dissipated. The book, as we have said before, is confined to the investigation of the ultimate average condition of the system, and does not discuss the processes of diffusion by which that ultimate condition is attained, such as the inter-diffusion of gases, the diffusion of momentum by viscosity, and the diffusion of energy by thermal conduction. These have been recently treated in a larger work,[1] to which we may have occasion to refer.

There are two very different methods of defining and investigating the state of a complex material system. According to the strict dynamical method the particles of the system are defined in any sufficient manner, as, for instance, by their co-ordinates at a given epoch, and the position of any particle at any other time is then defined by its co-ordinates, expressed as functions of the time, the form of these functions being different from particle to particle, and not necessarily continuous in passing from one particle to another which was contiguous to it in the initial configuration.

According to this method our analysis enables us to trace every particle throughout its whole course, and therefore we can apply the laws of motion in all their strictness.

[1] "Die kinetische Theorie der Gase in elementarer Darstellung, mit mathematischen Zusätzen. Von Dr. Oskar Emil Meyer, Professor de Physik an der Universität Breslau. (Breslau, 1877.)

The application of this method to systems consisting of large numbers of bodies is out of the question. We therefore make use of another method which we may call the statistical method, on account of its analogy with the methods employed in dealing with the fluctuations of a large population.

We divide the bodies of the system into groups according to their position, their velocity, or any other property belonging to them, and we fix our attention not on the bodies themselves, but on the *number* belonging at any instant to one particular group. This number is, of course, subject to change on account of bodies entering or leaving the group, and we have therefore to study the conditions under which bodies enter or leave the group, and in so doing we must follow the course of the bodies according to the dynamical method. But as soon as the process is over, when the body has fairly entered the group or left it, we withdraw our attention from the body, and if it should come before us again we treat it as a new body, just as the turnstile at an exhibition counts the visitors who enter without respect to what they have done or are going to do, or whether they have passed through the turnstile before.

The first mode of grouping the bodies of the system is to class those together which, at a given time, are in a given region of space. This is called grouping according to configuration, and what we learn from it is the distribution of the positions or co-ordinates of the bodies in space.

The second mode of grouping is that according to velocity. The best way to understand this is to suppose a diagram of velocities constructed by drawing from a given point as origin a system of vectors representing in direction and magnitude the velocities of the different bodies. The extremities of these vectors are called the velocity-points of the bodies to which they correspond, and by grouping the bodies according to the regions of the diagram in which their velocity-points lie, we learn from the numbers in the groups the distribution of velocities among the bodies.

In like manner we may form groups defined in any other way, as, for instance, those pairs of bodies whose distance from one another lies between given limits, and by confining within sufficiently narrow limits the values of all the properties of the bodies which form the group, we may consider all the bodies belonging to the group as practically in the same state. Whether at a given instant any body actually belongs to the group is, of course, another question.

The object of study in the statistical method is the probable number of bodies in each group. We may get rid of the idea of probability by supposing the system to continue under the same conditions for a very long time. During this time many bodies will enter the group, stay in it for a certain time, and then leave it. If we add together the times of residence within the group af all these bodies, and divide the sum by the whole time of observation, we obtain a numerical quantity which we may call the average number of bodies in the group. The longer the time of observation, the nearer does this number approach to what we have called the probable number of bodies in the group.

The average number of bodies in a group depends on

the limits which define the group, being, of course, greater when these limits are wide than when they are narrow. But it also depends on the character of the group, that is to say, the particular set of mean values of the conditions which entitle a body to be ranked in the group.

It appears from the investigation that if ϕ be any property of a body, such that if ϕ_1 and ϕ_2 are its values or two bodies before an encounter, and Φ_1 and Φ_2 its values after the encounter, and if under all circumstances $\phi_1 + \phi_2 = \Phi_1 + \Phi_2$, and if the number of bodies in each group varies as $e^{-h\phi}$, then the distribution of the bodies in the groups will not be altered by the encounters between the bodies.

Now if we make ϕ equal to the sum of the kinetic and the potential energy of each body, the quantity $\phi_1 + \phi_2$ is not altered, either by an encounter between the two bodies or by external forces acting on them; so that a distribution according to the values of the function $e^{-h\phi}$ will satisfy the condition of permanence.

The most general case is that given in the seventh proposition. The bodies are no longer supposed to be smooth rigid-elastic spheres, but molecules, that is to say, material systems consisting of any number of parts acting on each other with forces of any kind consistent with the principle of the conservation of energy. The molecules of any one kind are supposed to have m degrees of freedom, this number being, in general, different in different kinds.

It is also assumed in the enunciation that all the forces in the system are either forces tending to fixed centres and functions of the distances from these centres, or else forces acting between the parts of the same molecule, thus excluding forces acting between one molecule and another except during the encounter of two molecules. This restriction, however, does not appear necessary, and indeed it is easy to remove it.

For the result of the proposition is to prove that if we define the group (A) of molecules as consisting of those whose generalised co-ordinates (q) are between certain limits (q and $q + dq$), and whose generalised momenta (p) are between certain other limits (p and $p + dp$), then the average number of molecules in the group is—

$$A e^{-h(\chi + T)} dp_1 \ldots dp_m \, dq_1 \ldots dq_m,$$

where A is a constant which is the same for all groups of molecules of the same kind, but is different for different kinds of molecules in the same mixture, but h is the same for all kinds of molecules. χ is the potential energy, and T the kinetic energy of a molecule when in the state (A), and $dp_1 \ldots dp_m$ are the differentials of the components of momentum, and $dq_1 \ldots dq_n$ the differentials of the co-ordinates. The continued product of these differentials specifies the extent of the group.

By integrating this expression with respect to any one of the variables, we may ascertain the average number of molecules in a larger group, in which that variable does not form a ground of subdivision. For instance, if we integrate with respect to all the co-ordinates, we arrive at a group consisting of all the molecules whose momenta are between certain limits, or by integrating with respect to the momenta we form a group of molecules whose configuration lies within certain limits.

In this way we obtain two very important results:—

1. The average kinetic energy of a molecule is $\frac{m}{2h}$ where m is the number of degrees of freedom of the molecule. This is independent of the position of the molecule.

2. The average number of molecules whose configuration lies between certain limits is—

$$A e^{-h\chi} dq_1 \ldots dq_n,$$

where χ is the potential energy of the molecule, arising from forces either internal to the molecule or tending to fixed centres, but (according to Mr. Watson) excluding intermolecular forces.

But as our definition of a molecule is of the most general kind, nothing is easier than to take into account any intermolecular forces by simply including within our "molecule" all those molecules between which intermolecular forces are exerted.

For instance, there is nothing to prevent us from defining as a molecule a material system consisting of one atom in Sirius, another in Arcturus, and a third in Aldebaran. If the universe is supposed to have attained that condition of thermal equilibrium to which alone these propositions apply, the average kinetic energy of each of these atoms will be $\frac{3}{2h}$, because each has three degrees of freedom.

That of the system of three atoms will be the sum of the kinetic energies of the three atoms, namely, $\frac{9}{2h}$. We might obtain the same result from the consideration that this system has nine degrees of freedom.

The centre of mass of the three atoms is a mathematical point at an immense distance from any of them. It has, of course, three degrees of freedom, and the kinetic energy of a material particle whose mass is the sum of the masses of the atoms and which moves as the centre of mass does is $\frac{3}{2h}$.

The value of the kinetic energy of the centre of mass will be the same for any system of atoms provided that every atom of the system is liable to encounters with atoms not belonging to the system. Of course if we take into our "molecule" all the atoms of a material system unconnected with any other system, its centre of mass will not be agitated at all by the mutual actions of the atoms during their encounters.

And here we must notice a point to which Mr. Watson has adverted only in a note at the foot of p. 20—the definition of the motion of the medium as distinguished from the motion of agitation of the molecules. For this is connected with the weak point of the demonstration and shows us the way to strengthen it.

The weak point of the demonstration is the tacit assumption that the sum of the potential and kinetic energies of a pair of molecules is the only function which does not change during their encounter. For there are other quantities which are not altered by the mutual action of t o bodies, such as their masses themselves, the sum of their momenta resolved in any given direction, and their angular momenta about any fixed axis.

Hence if instead of $T = \frac{1}{2} M (u^2 + v^2 + w^2)$ we write in the expression for the distribution of velocities—

$$\mathfrak{C} = T - M\{Uu + Vv + Ww + p(wy - vz) + q(uz - wx) + r(vx - uy) + CM,$$

the distribution of velocities will still be a permanent one.

In this expression the quantities U, V, W, p, q, r, may have any values provided they are the same for the whole system of bodies, but C may be different for different bodies, because it is multiplied by the mass of the body, which is invariable. But we arrive at the same expression by substituting in T for u, v, and w the quantities—

$$u - U + qz - ry$$
$$v - V + rx - pz$$
$$w - W + py - qx,$$

or, in other words, by substituting for the absolute velocities of the bodies their velocities relative to a system of axes moving in the most general manner possible; that is to say, the components of velocity of the origin being U, V, W, and the components of the velocity of rotation being p, q, r, and at the same adding to T the quantity

$$\tfrac{1}{2} M\left\{(qz - vy)^2 + (rx - pz)^2 + (py - qx)^2\right\} - C',$$

which depends on the co-ordinates only and not on the velocity of the body.

We now see that the most general case of permanent distribution is when the system of bodies is contained in a vessel of invariable form which moves with constant velocity along a screw, that is to say, in which one point is moving along a straight line with constant velocity, while the vessel rotates about an axis passing through this point with constant angular velocity.

When there is rotation, we must subtract from the potential energy a term depending on the co-ordinates, which shows that the rotation produces an effect similar to that of a centrifugal force at right angles to the axis of rotation.

Returning to the general expression for the number of molecules in a group, we may make it yield us information of other kinds. Thus, if we wish to know the density of a particular gas at any given point in the mixture, we have only to make the limits of the group those of an element of volume, and we find the density proportional to $e^{-h\chi}$, where χ is that part of the energy of a single molecule which is due to external forces, such as gravity. In the case of gravity, χ is equal to $m g z$, where m is the mass of a molecule, g the intensity of gravity, and z the height. This leads to the ordinary expression for the density of a gas of uniform temperature in a vertical column, and it shows that in the ultimate distribution of a mixture of gases the density of each gas diminishes with the height according to its own law, that is to say that of the heaviest gases diminishes most rapidly, so that the proportion of the heavier gases diminishes with the height.

This law of the distribution of gases was asserted by Dalton as a consequence of his theory of gases, and numerous experiments have been made on air collected at different heights in the atmosphere in order to detect a difference in their composition, but we cannot say that such a difference has as yet been satisfactorily established.

The atmosphere, in fact, is eminently unfitted for testing the theory of the ultimate state of a mixture in equilibrium, for the inequalities of temperature in so large a body of gas produce powerful currents which continually carry masses of the mixture from one stratum into another. This tends to produce a uniformity of composition and a variation of temperature which are both of them contrary to our theory of the condition of equilibrium, and which seem to favour certain other theories.

Nor is the case much improved if, instead of the open atmosphere, we substitute a mixture of gases contained in a vertical tube. For in order to obtain a difference of composition at the top and bottom of the tube large enough for experimental verification, the tube must be at least 100 metres high, and it would take more than a year for the contents of such a tube to approximate by one half to their final distribution. In the mean time the slightest difference of temperature in the sides of the tube would produce currents which would tend to equalise the composition of the mixture. To verify the other result of our theory—the uniformity of temperature in the ultimate state of a vertical column—would be attended with still greater difficulties.

But it would be quite within the powers of experimental methods to verify the law of distribution of a mixture of gases in a rotating vessel. Let two bulbs be connected by a wide tube, say 10 cm. long, and let them be filled with equal volumes of hydrogen and carbonic acid, well mixed together. Let this apparatus be placed on a whirling machine, so that one bulb shall be close to the axis, while the other is moving at the rate of fifty metres per second. The same degree of approximation to the final state, which would take years in the long tube, will be effected in minutes in this small apparatus, and the proportion of carbonic acid to hydrogen will be about $\frac{1}{140}$ greater in the bulb furthest from the axis.

The clear demonstration of this proposition given by Mr. Watson is of great scientific value, for almost every one of those who have attacked the question with insufficient methods of investigation have come to the conclusion that the temperature would diminish in a vertical column as the height increases; and those who regard gaseous diffusion from a chemical rather than a dynamical point of view would probably expect the composition to be uniform at all heights.

But the profound scientific value of this proposition becomes more manifest when we make use of it in establishing the definition of temperature and the law of volumes of gases.

In Prop. II. of this book, which corresponds to the original form of the theorem, as I gave it in the *Philosophical Magazine*, January, 1860, two sets of spheres are completely mixed up together in the same vessel, and it is proved that the average kinetic energy of a sphere is the same for either set. We may then assert, as I did, that the two gases are at the same temperature because they are thoroughly mixed together. But this assertion has no scientific meaning, because we cannot test its truth by putting a thermometer first into the one gas and then into the other.

But if we now call to our aid a system of forces acting on the molecules and tending to fixed centres, we may obtain a result capable of experimental verification; for though we are not acquainted with natural forces acting exclusively on one kind of gas, we can calculate the effects of such forces.

Let us assume, then, that the forces are such that the potential energy of a sphere of the set N is much greater

in one part of a vessel, which we shall call B, than when it is in the part A, these two parts being separated by a stratum, C, within which the potential varies continuously. The medium consisting of the spheres N will be dense in A, it will become rarer in the stratum C, and there will be hardly any of these spheres in B.

Now let the potential energy of a sphere of the set N' be much greater when it is in A than when it is in B, and let it vary continuously from the one value to the other in the stratum C. Then the spheres of this set will be thickly scattered in B, will thin out in the stratum C, and will be very rare in A.

The two sets of spheres are thus kept in great measure separate in A and B, while free to exchange their kinetic energy by collisions within the stratum C.

Now by definition, the temperatures of two bodies are equal if, when the two bodies are placed in contact, their thermal state remains the same. We cannot apply this definition to the two sets of spheres in Prop. II., for they were inextricably mixed up together, but we have now got them almost completely separated from each other into two distinct regions. They are therefore practically distinct bodies, and we can test their temperatures separately.

Hence the statement, that the temperatures of two gases are equal when the kinetic energy of the centre of mass of a molecule is the same in each, is true, not only of gases mixed together, but of two pure gases in different parts of the same vessel.

If we assume that a powerful external force acts on each molecule tending to a fixed centre belonging to that molecule, each molecule will always remain very near its own fixed centre of force, and the assemblage of molecules will behave like a solid body. But forces of this kind are included among those considered in Prop. IV., so that the relation between temperature and the kinetic energy of the centre of mass of a single molecule must be extended even to solids.

Returning once more to the general expression for the average number of molecules in a group, we may make it yield us information with respect to the average number of sets of molecules which, at a given instant, are in a given configuration with respect to each other.

For instance, if two molecules act on each other, and if χ is the potential energy due to this action corresponding to a distance r, then the number of pairs of molecules whose distance is between r and $r + dr$ will be proportional to $r^2 e^{-h\chi}$. In the case of attraction, χ is negative, so that there will be a greater number of pairs of molecules within these limits of distance than there would have been if they did not attract each other. In the case of repulsion, χ is positive, so that the repulsion diminishes the number of pairs within the distance of repulsion. If the potential energy of a pair of molecules rapidly increases to an enormous value when the distance between their centres becomes less than a given quantity, the number of pairs which are within the given distance will be practically zero, and the molecules will behave like smooth rigid-elastic spheres.

By making the "molecule" include three or more molecules, and making χ the potential energy of this system, we may extend the theorem to the simultaneous encounter of three or more molecules, so that these cases, which were formally excluded in the earlier propositions, do not in any way interfere with the absolute generality of the final result.

The clear way in which Mr. Watson has demonstrated these propositions leaves us no escape from the terrible generality of his results. Some of these, no doubt, are very satisfactory to us in our present state of opinion about the constitution of bodies, but there are others which are likely to startle us out of our complacency, and perhaps ultimately to drive us out of all the hypotheses in which we have hitherto found refuge into that state of thoroughly conscious ignorance which is the prelude to every real advance in knowledge.

If we know from observation either the specific heat of a gas at constant pressure, or the ratio of its specific heats at constant pressure and at constant volume, we can determine the ratio of the rate of increase of its total energy to the rate of increase of the energy of agitation of the centres of its molecules. Now if the molecule has m degrees of freedom, its total kinetic energy is to the energy of agitation of its centre of mass as m to 3. It is probable that the internal potential energy of the molecule increases as the temperature rises, and this would make the ratio of the whole energy to that of agitation of centres greater than that of m to 3, so that if we know this ratio by experiment, we can assert that m cannot exceed a certain value.

For chlorine, ammonia, and sulphuretted hydrogen, m cannot exceed 6; for hydrogen, oxygen, nitrogen, air, carbonic oxide, nitrous oxide, and hydrochloric acid, it cannot exceed 5, and for mercury gas, according to the experiments of Kundt and Warburg, it cannot exceed 3.

Now Boltzmann has pointed out in a paper: "Über die Natur der Gasmolecüle" (Vienna Acad., December 14, 1876), that if the molecules were rigid-elastic bodies of any form, m would be 6, that if they were smooth figures of revolution, the velocity of rotation about the axis of figure would not be affected by the collisions, so that m would be 5, and that if they were smooth spheres, the three component velocities of rotation would each of them be independent of collisions, so that m would be reduced to 3, and these values are in striking agreement with the phenomena of the three groups of gases.

But before we accept this somewhat promising hypothesis, let us try to construct a rigid-elastic body. It will not do to take a body formed of continuous matter endowed with elastic properties, and to increase the coefficients of elasticity without limit till the body becomes practically rigid. For such a body, though apparently rigid, is in reality capable of internal vibrations, and these of an infinite variety of types, so that the body has an infinite number of degrees of freedom.

The same objection applies to all atoms constructed of continuous, non-rigid matter, such as the vortex-atoms of Thomson. Such atoms would soon convert all their energy of agitation into internal energy, and the specific heat of a substance composed of them would be infinite.

A truly rigid-elastic body is one whose encounters with similar bodies take place as if both were elastic, but which is not capable of being set into a state of internal vibration. We must take a perfectly rigid body and endow it with the power of repelling all other bodies, but only when they come within a very short distance

from its surface, but then so strongly that under no circumstances whatever can any body come into actual contact with it.

This appears to be the only constitution we can imagine for a rigid-elastic body. And now that we have got it, the best thing we can do is to get rid of the rigid nucleus altogether, and substitute for it an atom of Boscovich—a mathematical point endowed with mass and with powers of acting at a distance on other atoms.

But Boltzmann's molecules are not absolutely rigid. He admits that they vibrate after collisions, and that their vibrations are of several different types, as the spectroscope tells us. But still he tries to make us believe that these vibrations are of small importance as regards the principal part of the motion of the molecules. He compares them to billiard balls, which, when they strike each other, vibrate for a short time, but soon give up the energy of their vibration to the air, which carries far and wide the sound of the click of the balls.

In like manner, the light emitted by the molecules shows that their internal vibrations after each collision are quickly given up to the luminiferous ether.

If we were to suppose that at ordinary temperatures the collisions are not severe enough to produce any internal vibrations, and that these occur only at temperatures like that of the electric spark, at which we cannot make measurements of specific heat, we might, perhaps, reconcile the spectroscopic results with what we know about specific heat.

But the fixed position of the bright lines of a gas shows that the vibrations are isochronous, and therefore that the forces which they call into play vary directly as the relative displacements, and if this be the character of the forces, all impacts, however slight, will produce vibrations.

Besides this, even at ordinary temperatures, in certain gases, such as iodine gas and nitrous acid, absorption bands exist, which indicate that the molecules are set into internal vibration by the incident light.

The molecules, therefore, are capable, as Boltzmann points out, of exchanging energy with the ether.

But we cannot force the ether into the service of our theory so as to take from the molecules their energy of internal vibration and give it back to them as energy of translation. It cannot in any way interfere with the ratio between these two kinds of energy which Boltzmann himself has established. All it can do is to take up its own due proportion of energy according to the number of its degrees of freedom.

We leave it to the authors of the "Unseen Universe" to follow out the consequences of this statement.

J. CLERK MAXWELL

OUR BOOK SHELF

Report on the Progress and Condition of the Royal Gardens at Kew during the Year 1876. (Clowes and Sons.)

SIR JOSEPH HOOKER'S annual report on the Royal Gardens, Kew, for 1876, has just been issued. It is a pamphlet of some thirty-three pages, and is a considerable improvement on the reports of former years. It deals most fully with new plants of economic interest, whether such have been actually received or sent from the Royal Gardens, or have formed the subject of correspondence with foreign or colonial governments. It is eminently satisfactory to know that such useful plants as the Para rubber (*Hevea brasiliensis*), the ipecacuanha (*Cephælis ipecacuanha*), the Liberian coffee (*Coffea liberica*), and others, have been most successfully introduced into India and other countries, through the instrumentality of Kew. Of the 70,000 seeds of the *Hevea* received at Kew about the middle of June last year, all of which we are told were at once sown, and though closely packed together, covered a space of over 300 square feet so soon as August 12th following, upwards of 1,900 living plants, raised from these seeds, were transmitted to Ceylon in thirty-eight Wardian cases, 90 per cent. of the whole consignment reaching Dr. Thwaites in excellent condition. So rapid was the germination of these seeds at Kew that some had actually started into growth on the fourth day after sowing, and many in a few days reached a height of eighteen inches. It has been arranged that these young plants shall "be nursed and established in Ceylon for subsequent transmission through the Indian Gardens to Assam, Burma, and other hot damp provinces of India proper." Besides those sent to India, smaller quantities of plants have also been despatched to the west coast of Africa, Burma, Dominica, Jamaica, Java, Queensland, Singapore, and Trinidad. With regard to ipecacuanha, though Dr. King reports that he fears it cannot be grown so far north in India as Bengal, it is nevertheless in some situations capable of rapid and extensive cultivation, and the roots grown in India have been proved to be quite as efficacious in a medicinal point of view as those from the best districts of South America. In the matter of Liberian coffee, the wide and general extension of this new kind in coffee-growing countries bids fair, in many parts, to entirely supersede the old and better known *Coffea arabica*. Sir Joseph Hooker reports the receipt of numerous favourable notices of the plant, and quotes "two from opposite sides of the world," namely Ceylon and Dominica. With reference to diseases affecting coffee plants—which it is hoped the more sturdy habit of the Liberian kind will help it in some measure to resist—a very exhaustive notice is furnished, which is not only of much interest in a scientific point of view, but cannot fail to be valuable to coffee-planters themselves. It will, moreover, no doubt be the means of causing more careful observations to be made by residents on coffee estates or in coffee-growing countries into the nature and habits of diseases which are still more or less obscure.

Considerable additions are reported to the Museums and Herbarium, the new building for the accommodation of the latter collection being now in a very advanced state. The new Laboratory, which has been erected at the expense of T. J. Phillips Joddrell, Esq., is reported as having been completed during the year, and though not fully provided with the necessary equipment at the time the report was written, has been already, as our readers are aware, used by Dr. Tyndall in several of his recent experiments and researches.

Two new features of the report which we have not already mentioned are—first, the introduction of plates, one being a figure of the new Liberian coffee plant, and the other a view and ground-plan of the Laboratory; and second, the publication of the report, at a charge of sixpence, by Messrs. Clowes and Sons.

Natural History Transactions of Northumberland and Durham, vol. v., part 3. (Williams and Norgate, 1877.)

THIS part is by no means the least valuable of these transactions; on the contrary, it will rank high, owing to the contributions of Dr. Embleton and Mr. Atthey on the structure of the Labyrinthodonts, and the eight excellent plates by which their papers are illustrated. The illustrations of Loxomma and Anthracosaurus are as complete and instructive as any that have yet been pub-

lished of British Labyrinthodonts. The authors, however, do not recognise the articular surfaces on the exoccipitals of Loxomma as the two condyles; and they speak of a concave articular surface as taking the place of a condyle or condyles on the basioccipital bone. The condyles in all Amphibia are produced by the exoccipital bones, and such a character is not a special evidence of the affinity of Loxomma with fishes. The number also contains several interesting papers on local natural history and antiquities, and the address of the president, the Rev. G. Rowe Hall.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Inflexible"

MY attention has been called to an article on the *Inflexible*, in NATURE (vol. xvi., p. 201), and I shall be much obliged by your inserting a few remarks, which I shall make as short as possible. On the general subject of the article I do not propose, nor would it be proper for me, to say a word. I am concerned only with the concluding remarks of the writer on a letter of mine to the *Times*. Nothing has appeared to me more astonishing than the use, or rather abuse, which is occasionally made of the report of Lord Dufferin's committee on ships' designs. If their authority can be claimed for any statement, I see on all sides a readiness to claim it. Should anything they have said militate against a favourite view, their authority is depreciated, and a comparison is sometimes invidiously drawn between the supposed opinion of the majority and that of an intelligent minority. Now if I be included with the unintelligent majority, I am quite content to find myself in such good company; but if, on the contrary, I am included in the minority, I utterly and absolutely refuse a compliment at the expense of my distinguished colleagues, with whom I shall always esteem it no small honour to have served. In fact I am not aware of any scientific point on which that committee was not unanimous. The writer of the article in question in common with many others, seems to have entirely mistaken the position of that committee. He seems to think their duty was to make their report a treatise on Naval Architecture. The absurdity of such a notion is apparent on the face of it. In fact they were required to give an opinion on certain designs of ships submitted to them as to their being in accordance with the latest developments of the theory of naval architecture. It was no part of their duty to descant on the principles which were successfully applied in such designs; but undoubtedly if they observed that in any direction caution was necessary, they were bound to remark it. In none of the designs was there any indication of a tendency to curtail initial and maximum stability of their due proportions; had there been they would certainly not have failed to call attention to it. But while they found the design of the *Devastation* in all respects sound, they yet thought it desirable that the range of stability should in future designs be somewhat enlarged. In recommending such enlargement they by no means committed themselves to any such absurd *dictum* as the writer imagines—that range of stability is all that is requisite for the safety of a ship. But as I have already said, to have laid down all the other requisites of a good ship would have been to write a treatise.

Again, whatever credit according to some, or discredit according to others, is due to the design of a ship like the *Inflexible* with an armour-plated central citadel with unarmoured bow and stern, that credit or discredit cannot be justly imputed to the committee. Mr. Reed, in his evidence, had brought a design with some of the leading features of such a ship before them, and it occupied a considerable share of their attention. Now what do they say on this subject?—"It is not by any means certain that some method may not be devised of securing the requisite reserve buoyancy by other means than armour plating." And after giving a sketch of what such a ship would be, they conclude thus:—"In the absence of any practical experience of the effect of large shells or of torpedoes upon such a structure as we have in view, it is impossible to say with confidence that the object aimed at would be thus attained, but, *if it were*, consequences of so much importance and value would follow that we think it right to *indicate this line of inquiry as worthy of experimental investigation.*"

How far such a bare suggestion of experimental inquiry is from the recommendation of such a structure for adoption must be evident to your readers without further comment.

United University Club, Pall Mall, July 20 — JOSEPH WOOLLEY

[The above letter from Dr. Woolley is what might have been expected from a man of his eminence in the science of naval architecture, writing under the restraint of his nomination by the Government to a membership of the Committee which is to report upon the stability of the *Inflexible*. It is no doubt to the concluding words of our first article on this subject (NATURE, vol. xvi. p. 203) that Dr. Woolley's letter refers, and we at once admit that there is very great force in the argument which he now employs. The particular point in question is a very simple one. In his letter published in the *Times* of July 19, Mr. Barnaby wrote:—"According to our estimate the ship, when fully armed, stored for fighting, and manned, will have, independently of the unarmoured ends—*i.e.*, supposing them not to exist—a range of stability of 48 deg. The Committee on Designs considered that 40 deg. was sufficient range for a seagoing unmasted ship." On the following day a letter appeared from Mr. Reed commenting on the impropriety of assuming the *non-existence* of the ends, pointing out that it was 50 deg. and not 40 deg. that the Committee spoke of as the *minimum* angle of vanishing stability, and adding that when the Committee put forward "*range* of stability" as "the one measure of safety" to be considered, "they stated the most dangerous doctrine which probably has ever been propounded in connection with the science of naval architecture." Now, on reconsidering the whole question, we are inclined to think that these words were not, in point of fact, quite fair to the Committee, because there was probably no member of the Committee who would have asserted or admitted that "range" was the one and only measure of safety to be considered. Dr. Woolley, Mr. Froude, Sir W. Thomson, and probably some other members of the Committee, doubtless knew perfectly well that the length of G Z from point to point was not only as important as "range," but far more important in all cases of limited range; and it is now obvious, with the present letter of Dr. Woolley before us, that the absence of any reference to the fact is attributable to the limited extent of the Committee's inquiry. There is great force in the remark that it was no part of the duty of the Committee to compose a treatise on naval architecture. On the other hand we are bound to deny that our remarks were penned under a contrary impression. Our view is that the use to which Mr. Barnaby has put the Report of the Committee proves that the scientific men who composed it would have done well to have employed more guarded language, and to have recognised in some manner the insufficiency of range only as a measure of safety. When they are found speaking of a certain angle of vanishing stability as being "sufficient to ensure the safety" of ships, it must be admitted, even by Dr. Woolley and his colleagues, that some risk of misconstruction was incurred. That misconstruction, or perhaps we ought in this case to say misuse—or even "abuse," as Dr. Woolley expresses it—has occurred in the present case is manifest, because Mr. Barnaby seized hold of the Committee's dictum as to range, and ignored altogether the very serious question of the amount of the stability. What makes the matter more important than usual in the present case is that the curve of stability due to the citadel of the *Inflexible* only is, no doubt, a low and flat curve, G Z being everywhere so small that in order to bring the stability up to a safe amount its range

would have to be very greatly extended. It was, no doubt, improper of Mr. Barnaby to make the use he did of the Committee's words, and Mr. Reed took no pains to credit the Committee with anything beyond what was written; but Dr. Woolley is, we think, a little forgetful of the fact that what Mr. Reed wrote, and what we have since written, has had to be said in presence of the circumstance that in a matter of the gravest public importance a free use of the Committee's words has been made by a high authority for the purpose of claiming for the *Inflexible* public confidence in her stability on the ground of range only. It is satisfactory to learn, however, on the undoubted authority of Dr. Woolley that the Committee, whatever its language, not only inetnded to give no countenance to the doctrine that the *Inflexible* would be proved safe if only she were shown to possess sufficient range of stability, but individually and collectively would consider such a doctrine as altogether absurd.

On the second portion of Dr. Woolley's letter, we entirely concur with him. We have read over again both the evidence and the reports of the Committee of Designs, and we cannot find the smallest justification for the assumption that, right or wrong, the Committee on Designs is responsible for this ship's design. The case to the contrary is absolutely clear and unquestionable. Mr. Reed placed before the Committee the outline ideas of a ship of this description, but making it a *sine quâ non*—let it in justice to him be said—that the ship should not depend "in the least degree" upon the ends, and that the stability of the citadel should be so ample as "to make it a matter of perfect indifference how much the ends might be knocked about by shot and shell." He spoke of the ends as being filled with water, and thus converted into a sort of tanks, and it most naturally occurred to the committee to suggest whether cork or some metallic cellular material, might not with advantage be employed to take in some degree the place of water, a proposal which Mr. Reed thought well worth consideration and trial. Beyond this the Committee did not go in their report, as the quotations cited by Dr. Woolley clearly show; on the contrary, by recommending the course of experimental investigation which they advised they plainly showed that, in their opinion, sufficient grounds for depending upon cork, &c., for stability did not exist, and could not be shown to exist except by large and well-considered experiments. Mr. Barnaby roundly asserts that the Committee "did not agree with Mr. Reed's view as to the necessary dependence of the ship upon her armoured citadel for her floating power;" but the extracts from the Committee's Report which he adduces in support of the statement by no means bear it out. The Committee, for some reason or other, advert to Mr. Reed's plan without mentioning his name, but, while nowhere implying any dissent from his main principle, they plainly enough indicate that armour should be employed to sufficiently protect buoyancy and stability, unless "other means than armour-plating" could be found and proved effectual.

We shall defer to Dr. Woolley's very proper wish to restrict his remarks to the two points above considered, and shall in no way seek to connect them with the general question upon which he has been appointed a judge. We may be permitted to observe, however, that whatever the result may be, it is a satisfaction to us to find that the Committee consists of gentlemen who are in large part not merely masters of the science of the stability of floating structures, and raised high by their individual repute above the suspicion of partisanship, but who also, by serving on the Committee of Designs of 1871, acquired very special fitness for promptly considering the *Inflexible* case. They will know how to go directly to the questions at issue, and after ascertaining what stability the ship actually possesses without aid from cork, or canvas, or other devices, and what she possesses with such aid, they will be able to declare with scientific confidence and precision whether it is or is not sufficient, for they are themselves the authors of the very standards by which that issue must be decided. Nor will they forget that whatever demands for stability existed in 1871, still greater demands now exist when we have the First Lord of the Admiralty, in his place in Parliament, claiming for this very ship the ability to float and fight even after three successive blows from Whitehead torpedoes. If the result should be a disproof of our views of the subject, we know that that disproof will be based upon scientific grounds that will commend themselves to impartial minds. If the result should be to require that additional stability shall be provided in such ships, a great public good will have been accomplished. We need not say which result we anticipate.—ED. NATURE.]

The Manufacture of Leading Articles

THERE is a good old story told of a country editor who once met a pressing demand for copy in a singularly ingenious manner. At the moment of going to press, it was found to the consternation of the printer that a whole column was lacking. What was to be done? The whole staff was in confusion at the unexpected discovery; the editor alone preserved his wonted coolness. Sending for a copy of the *Times*, he clipped therefrom one of the leaders and ordered it at once to be set up in type, prefaced by the words "What does the *Times* mean by this?"

This story recurred to me with some force on reading on the front page of *Land and Water* last week, an article on Soldiers' Food in War; for the original, bearing my signature, appeared on the front page of NATURE (vol. xvi. p. 157). In this case, however, my other self seems to have had more time on his hands than the country editor, since the article in question has been paraphrased in parts, still with such care as not to destroy the identity. H. BADEN PRITCHARD

July 24

The Fish-sheltering Medusa

PROBABLY the species of fish to which Mr. Lawless refers as seeking shelter under the swimming-disc of *Aurelia aurita* (NATURE, vol. xvi. p. 227) is *Merlangus carbonarius* (Cuv.), popularly called boat-fish. At least I have seen the fry of this species behaving as Mr. Lawless describes.

The observation stated in the following words appears to me one of great interest:—"Occasionally the Medusa turned in its pulsations, so as to bring the umbrella undermost, when the fish would shoot hastily out, but the Medusa had no sooner righted itself, than the fish returned." Now, if this occasional turning on the part of the Medusa was not merely accidental, but, as Mr. Lawless implies, a reflex act performed with the view of escaping from the irritation occasioned by the fish, the fact would show that the marginal ganglia of *Aurelia aurita* are so far co-ordinated in their action as to enable the animal to steer itself in any required direction. For my own part, I have not as yet been able to satisfy myself that such ganglionic co-ordination occurs in any species of covered-eyed Medusa; so it would be well worth while if Mr. Lawless could repeat his observation a sufficient number of times to exclude the supposition of the somersaults being merely fortuitous.

I may take this opportunity of saying that the cut which illustrates the abstract of my lecture on p. 232 of the same issue of NATURE as contains Mr. Lawless's letter, is intended to represent the species of Medusa to which he refers, viz., *Aurelia aurita*. The cut is about $\frac{1}{8}$ natural size.

GEORGE J. ROMANES

Phyllotaxis

I HAVE noticed in the laurel and the Spanish chestnut species, in which the leaves have normally a distichous arrangement, that when a vigorous shoot takes a vertical direction—for example, after the stock has been cut down near the ground—the leafage of such a shoot is often quincuncial. The phenomenon suggests three possible interpretations. Is this to be regarded as a fixed adaptive habit, the spiral phyllotaxis being the fittest for the upright, the two-ranked for the more numerous lateral twigs? Or are the exceptional instances endeavours after greater economy of space in the packing of the buds? Or, finally, ought we to discern in the peculiarities of the more vigorous shoots a reversion towards some ancestral condition? W. E. HART

Drumaweir, Greencastle, July 20

Printing and Calico-Printing

As all that I am ever personally concerned to know is the truth of a matter, I am glad to stand corrected by the writer of the article on Calico-Printing in the "Encyclopædia Britannica." The claim I made, however, for the author of the "Natural History of Enthusiasm" was not my own invention; and it would be of interest, I think, to the many who must still, even in our day, revere his memory, to know more fully and accurately what it was that engrossed so many years of his valuable life, and what, if any, have been the practical results.

Bregner, Bournemouth, July 23 HENRY CECIL

THE VISIT OF THE BRITISH ASSOCIATION TO PLYMOUTH

THERE are very good reasons for anticipating that the Plymouth meeting of the British Association will be at least up to the average in interest and success. Indeed, in some respects it is anticipated that it will be unusually attractive; and the attendance is likely to be much larger than from the distance west was at first thought probable. And as the details of the local arrangements approach completion so do the outside attractions, rendered available for the pleasure or information of the visitors, increase and multiply. The Government authorities have kindly consented to render every facility in their power for the inspection of the great establishments which constitute Plymouth and Devonport one of the chief arsenals in the world. The dockyard and Keyham yards will be freely open; those who desire to inspect the famous biscuit machinery at the Royal William Victualling Yard will be enabled to do so; there will be gunnery practice and probably torpedo practice also to be witnessed on board the *Cambridge*. The Breakwater of course can be seen at any time from Plymouth Hoe, and visited whenever weather will permit, which unless a gale is blowing it always does. The Breakwater Fort, reared on an artificial island of stone in the Sound immediately within the Breakwater, granite-capped and iron-plated, is by far the most interesting of the great chain of forts wherewith the Three Towns are girdled, and this too, it is hoped, will be open to inspection. The Eddystone will be visited on the Saturday, as already stated; but it must be understood that it is by no means certain that a landing can be effected. In fair weather, even, there is at times such a swell there as to render landing difficult, and even dangerous, while in rough weather it is impossible.

The excursion arrangements have been somewhat extended since our previous notice. On the Saturday, in addition to the excursions to the Eddystone, Lee Moor, and Iskeard for the Caradon Mines, it is now proposed by the citizens of Exeter to invite a large party of the members to this famous city, which abounds in objects of antiquarian interest, and which is noted for the hospitality with which it receives its guests. Iskeard too, is moving in the same direction; and the proprietors of Lee Moor Clay Works, Messrs. Martin, intend to make provision also for their visitors. After the clay works have been seen and justice done to the luncheon, there will be ample opportunity for a delightful ramble on Dartmoor. Shell Top and Pen Beacon, with their magnificent views and prehistoric remains, are within very easy distance, and good walkers will have the opportunity of enjoying some of the most romantic scenery in Devon, in the valleys of the Plym, and other moorland rivers.

The excursion arrangements for the Thursday remain unchanged; but there has been a considerable addition to the list of available attractions. The engineers of the party, through the kindness of Mr. Margary, engineer of the Great Western Railway for the district, will be enabled to inspect the Royal Albert Bridge to their heart's content—even to a scramble through the tubes. The great granite works and granite quarries of Messrs. Freeman at Penryn; the mines of Dolwath, Tincroft, and Carn Brea (by the kindness of Capt. Josiah Thomas and Capt. Teague); the pneumatic stamps of Mr. Husband, at the Hayle Foundry; the tin smelting works of Messrs. Bolitho, at Penzance, will all, by the courtesy of their proprietors, be available to be visited by members of the Association. And as the Earl of Mount-Edgcumbe has kindly opened his magnificent park and his romantic mansion of Cotehele, so Sir John St. Aubyn permits his famous and historic residence, St. Michael's Mount, to be visited by those members of the Association who may find their way so far west.

The public museums of the two counties will, we believe, be all open to the members. The chief are that at Exeter, that of the Royal Cornwall Institution at Truro, and that of the Royal Cornwall Geological Society at Penzance. Mr. C. C. Ross, of the latter town, has one of the best private collections of minerals in the West of England, and will gladly show it to all who feel interested in mineralogy. Then there are the museums of the Torquay Natural History Society, the specialty of which is its Kent's Cavern collection, which will form one of the attractions of the Torquay excursion, and the Museum of the Plymouth Institution, in which will be found a magnificent collection of flint implements and weapons lent and arranged for the occasion by Mr. Brent.

There will be several local papers contributed to the various sections, but the list is hardly likely to be so long as at the meeting of 1841, when the local contributions were unusually numerous.

The Pharmaceutical Society will, as usual, hold their meeting immediately prior to the meeting of the British Association at Plymouth, and the Mineralogical Society and the Society of Public Analysts, will also meet at Plymouth during the Association week.

THE GORILLA

SINCE Monday last the young gorilla from the Berlin Aquarium has been exhibited, during most hours of the day, at the Westminster Aquarium, in company with a chimpanzee. This is the first occasion on which a living gorilla has been publicly exhibited in this country as such, an earlier specimen some years ago, in a travelling menagerie, having passed for a chimpanzee during its life-time.

The gorilla, which is about three years old, appears in excellent health, and differs most strikingly from its companion in the blackness of its face and extremities, the smallness of its ears, the shortness of its muzzle, the great development and breadth of the alae of its nose, the shortness and softness of its thick-set body-hair, the presence of a frontal hair-tuft, the breadth and flatness of its back, which is also capable of greater backward bending, the smallness of the four outer toes, which are free for but a short distance, the breadth of its hands, and the massiveness of the nape of the neck. The conjunctiva is black, and the eye intelligent. We think that no one interested in natural history should lose the opportunity of seeing this particularly interesting Anthropoid ape.

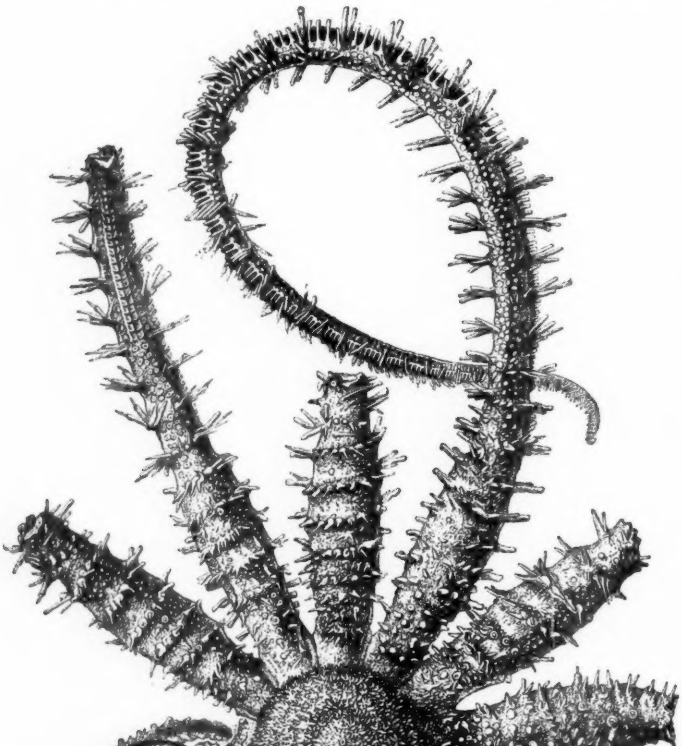
BRISINGA

NEARLY a quarter of a century ago the celebrated Norwegian poet and naturalist, P. Chr. Asbjörnsen, was dredging in the interior of the picturesque Hardangerfjord, when, at a depth of about 200 fathoms, the dredge brought up a wonderful new star-like Echinoderm, quite unlike any form that had been up to that moment described. From a little circular disc of about an inch in diameter there issued eleven spreading arms or rays upwards of a foot each in length. These were armed along the edges with several rows of long spines; these arms, while standing near together at their base, generally taper away gradually to their tips. The colour, though variable, was, on the upper or dorsal surface, of a more or less red hue and paler, often to whiteness, on the under surface. On the lower surface of the disc, and occupying the central space, is seen the mouth-like aperture of the alimentary system, and spreading away from it along the centre of each ray-like arm, are the deep ambulacral furrows, so called because from these furrows issue the ambulacra or water-feet. These form two uninterrupted rows, and are flanked by several palisades of strongly-developed spines, the outer ones being the longest. All these spines are enveloped in an integument which is covered with strange-looking Pedicellariæ.

Only an instant's glance at this brilliant novelty was vouchsafed to the poet-naturalist; for beneath his glance the star-fish, thus brought up to quarters new to it, threw off all its arms, and what was once a thing of beauty became now a tangled mass of writhing arms moving away from the disc that had so long borne them company.

From living in great darkness and in the tranquil depths of the ocean's bosom, the being brought so suddenly up into the bright sunlight and to the agitated movements of that ocean's surface was too great a change and too severe a shock, and the catastrophe just mentioned was the consequence. To Asbjörnsen, however, this thing of beauty seemed like a link in the chain of the past. In its unlikeness to most recent forms of star-fishes he saw its connection with certain fossil forms, and in its brilliant sun-like form he was reminded of the "Brising" which, according to ancient Norwegian tradition, was concealed by Loke in the abyss of the primeval ocean, but which had so long served as the ornament to cover the breast of the god Freya, and he gave the name of Brisinga to the new genus.

From the number of its arms Asbjörnsen called this new species *B. endecacnemos*, and until quite recently it was the only species known. In one locality of the fjord—Hesthammer—its occurrence cannot be considered as very rare, it has only been met with on rocky bottoms and at depths of from 200 to 400 fathoms. It has also been dredged by Prof. G. O. Sars some miles north of Bergen; by Sir Charles Wyville Thomson in the North Atlantic, and off the west coast of Portugal by Mr. Gywn Jeffreys, an account of the finding of these latter specimens will be found in Prof. Sir C. W. Thomson's most interesting work on the "Depths of the Sea."



Brisinga coronata, G. O. Sars. Natural size.

A second species of this genus was in 1869 and 1870 brought up by Prof. G. O. Sars at the fishing-place Skraaven, in Lofoten, from a depth of 300 fathoms, and this species has also since been found in the great depths of the Atlantic Ocean; for this species the name *B. coronata* has been selected by Sars, and this name has been adopted by Thomson, from whose work the accompanying beautiful woodcut of this species has been borrowed. It represents the animal as seen from above; five rudimentary arms in one series take the place of those that have been lost, for in this species the number of arms varies from nine to twelve.

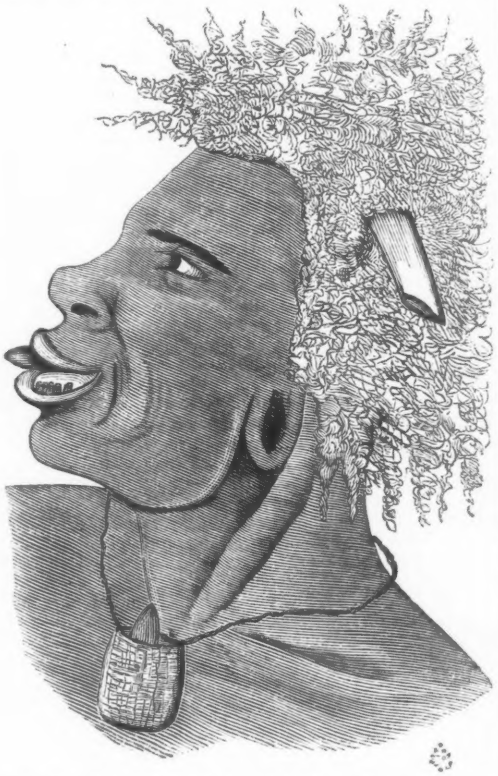
This new species has been made the subject of an elaborate memoir by Prof. G. O. Sars, in which memoir the structure and affinity of the genus is also fully discussed. It has been published as the University of Christiania's programme for the latter half of 1875. With the aid of a tolerably large number of fresh specimens and by repeated careful dissections, Sars has been enabled to ascertain most of the points in connection with its histology, and seven plates assist in illustrating the structures described. He considers the functions of the remarkable little organs called Pedicellariæ, which occur in most surprising number in both species of Brisinga, as that of seizing and holding fast the objects which come in contact with them, those that are found on the dorsal surface, thus acting as protectors to the thin skin; and those on the lower surface acting in the service of alimentation.

Among living star-fishes Brisinga seems to stand isolated, coming perhaps nearer to Pedicellaster; with the oldest known fossil star-fish Protaster, it shows close affinities, but would seem to be older and less specialised, and if so it would then be the most primitive as well as oldest form known of Star-fishes. It must therefore be kept in a family of Echinoderms by itself which may be called *Brisingidæ*.

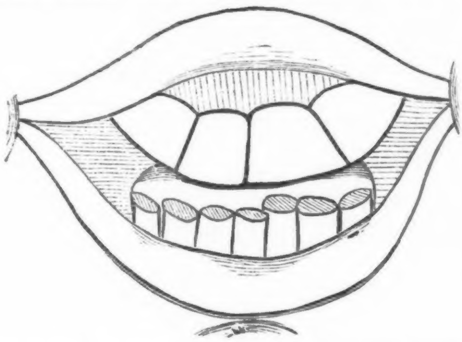
E. P. W.

A REMARKABLE DEFORMITY OF THE TEETH AMONG THE INHABITANTS OF THE ADMIRALTY ISLES[1]

THE Russian traveller, M. Miklucho-Maclay, in the course of recent travel in Melanesia, has noticed among the natives of the Admiralty and Hermit Isles a remarkable peculiarity in the teeth, the upper incisors



projecting "shovel like," almost horizontally, and to such a degree as to extend even beyond the lips when the mouth is closed. The breadth, moreover, of one of these



teeth is at times so great as to equal its visible length; being in the specimen figured as much as 19 millimetres to 16 mm. of length, measured, of course, from the edge of the jaws, not from the extremity of its fang. As all the teeth have a blackish polish, due to the prevailing habit of betel-chewing, the mouth presents a somewhat ghastly appearance. M. Miklucho-Maclay has nowhere else met with a similar deformity of the teeth, but heard of such, when on the peninsula of Malacca, the race in which it occurs being called "orang-gargassi."[2]

J. C. G.

[1] See a note contributed to the *Illustrirte Zeitung* of Leipzig by M. Miklucho-Maclay.

[2] *Man-devil. Orang* is the usual Malay word for man, while *gargassi* is equivalent to the German *Quälgeist*, tormenting spirit.—J. C. G.

RAINFALL AND SUN-SPOTS

WE have received the following communications having reference to Gen. Strachey's paper on the above subject, which we printed some little time back.

The conclusions of Mr. Meldrum as to a relation between the amount of rainfall and the frequency of sun-spots have become a subject of much interest with reference to the possibility of being prepared for such a deficiency of rain in India as may result in a failure of crops and consequent famine. That the varying yearly rainfall at Madras showed on the whole a rather marked agreement with the sun-spot period, has been known to me for some time, and Dr. Hunter has lately made an examination of the amounts relatively to an eleven-yearly cycle which has excited marked attention on account of its possible practical application. Gen. Strachey has made a discussion of the Madras observations in a paper read before the Royal Society, a full abstract of which has appeared in NATURE (vol. xvi., p. 171). He has sought to show that there is no evidence in the Madras observations of periodicity at all; and that if the rainfalls for each of the sixty-four years were written on slips of paper and drawn from a bag, so that the first amount drawn should be placed to the first year (1813), the second to the next year (1814), and so on, as well-marked a result would be obtained as is shown by the quantities observed in their respective years. This conclusion he founds on the following method, which he terms a "true criterion of periodicity."

If the differences of the rainfall for each year from the mean of the whole sixty-four years be taken, and the mean of all these differences (without respect to sign) be called the *general* mean difference; if we arrange the yearly rainfalls in horizontal series of eleven successive years (there will be six such series nearly), and the means for the first, second . . . . years of the series be taken, these quantities (periodic means) will show the mean variation in the period of eleven years, if any such exist. If now the differences of the yearly rainfalls from the periodic means for the corresponding years be obtained, the means of these, irrespective of sign, may be called the *periodic* mean differences. In the case of the Madras rainfalls Gen. Strachey finds—

The *general* mean difference = 12·4 inches,
The *periodic* ,, ,, = 11·2 ,,

and his true criterion of periodicity, though not so definitely stated as might be wished when so important a rule is proposed, appears to be that if there be no periodicity the variation in the periodic means will tend to disappear in a sufficiently long series of observations and the *general* and *periodic* mean differences be identical. It seems to me that the disappearance of a variation in the periodic means is here the true criterion of no-periodicity; but though a very large variation exists in the case of the Madras observations, yet Gen. Strachey concludes that since the *periodic* and *general* mean differences agree so nearly, there is no tendency to periodicity shown in the Madras observations.

As an illustration of the true criterion, Gen. Strachey

takes the two hourly observations of the barometer for five successive days at Madras, and shows that—

The *general* mean difference = 0·030 inches,
The *periodic* „ „ = 0·014 „

Here, he says, a true period existing, the *periodic* mean difference becomes much less than the *general* mean difference.

I shall now venture to show that this is no criterion of periodicity. If we represent variations of any quantity for a given time by a curved line, and if we have several such lines of exactly the same form placed one over the other, a straight line passing through the curves, with as much space between the straight and curved lines above as below, will represent the *general* mean. In a simple curve of two branches the *general* mean difference will be nearly one-fourth of the amplitude of the oscillation; while, as all the oscillations agree with each other, and therefore with the mean oscillation, the periodic mean difference will be zero. If, however, we displace the individual curves so that as many shall be above as below their mean, both the *general* and the *periodic* mean differences will increase, and the difference between these quantities will diminish, till the individual curves are so separated from the mean that none of them is cut by it, when the two mean differences will be *equal:* between this case and that of general coincidence the two mean differences will have values which will differ more or less from each other, according as the individual curves are nearer to, or more remote from, the mean; and the ratio of the one mean difference to the other will tend to a constant value as the number of cycles increases, a ratio which will depend for its value on the mode of distribution of the individual curves and of the irregular deviations from the mean.

Gen. Strachey's illustration is from a case approaching coincidence; hundreds of cases, however, may be found of the other class, especially when, as in this instance, only a few periods are in question. Thus, taking two hourly observations of the barometer at Simla during six days in the beginning of January, 1845, I find—

The *general* mean difference = 0·0634 inches,
The *periodic* „ „ = 0·0615 „

and if the last day of the six be omitted so as to have an *odd* number of days, I find—

The *general* mean difference = 0·0656 inches.
The *periodic* „ „ = 0·0634 „

Gen. Strachey's conclusion from the Madras rainfall observations is in fact that because the *periodic* mean difference was only one-tenth less than the *general* mean difference, there was no evidence of periodicity whatever; here we have a large and regular semi-diurnal period (the whole mean range being 0·070 inch) where the *periodic* is not one-thirtieth less than the *general* mean difference.

I may add that when the true sun-spot period of ten and a half years is employed for the Madras rainfall observations, I find—

The *general* mean difference = 12·4 inches,
The *periodic* „ „ = 10·2 „

quantities which differ by five times as much as those found for the true periodic variation of the barometer at Simla.

I have taken the variation chosen by Gen. Strachey to illustrate this question, but the fact that the difference of the *general* and *periodic* mean difference is no criterion of periodicity might have been shown equally well with cases more resembling that of the rainfall, where the irregular variations are large compared with those following a known period; I cannot here, however, enter into details and notice only the objections offered by me to Gen. Strachey's paper when it was read before the Royal Society.

It would be easy to show that the Madras rainfall observations, *taken alone*, give results which are remarkable in several respects. Thus, 1st, They show a *mean* oscillation larger for about ten (nine to eleven) years than for any other duration. 2nd. When the mean variations for a period of ten and a-half years are represented by a function of sines they give the yearly *mean* rainfall (y) in the period, $y = 6{\cdot}2 \sin(\theta + 310^\circ)$, showing the large range of 12·4 inches. 3rd. This representative equation gives the epochs of maximum rainfall in the years of maximum sun-spots, or as nearly so as would be given by the mean sun-spot areas represented by a similar expression.[1]

On the other hand, the irregularity in the amount of rainfall from year to year is so great that the probable error of the periodic means is too considerable to give any great weight to this result alone.[2] When observations during a sufficiently large number of cycles have been obtained, so as to make the probable error of the means small compared with the range of the periodic variation, then there will be a general acceptation of Gen. Strachey's remark: "It is hardly conceivable that there should be a coincidence with the sun-spot period, such as is supposed to be found at Madras, based on any physical cause which should not in some way be discernible in the rainfall at Bombay and Calcutta" (NATURE, vol. xvi. p. 172). He has then taken *five* cycles of eleven years' rainfall at Bombay, and *four* cycles at Calcutta, and testing them by his criterion he obtains results quite similar to that for Madras.

I have only the periodic *means* for the five eleven-yearly cycles at Bombay now before me, but seeking from these the representative equation of sines as for Madras, and repeating the latter for comparison, I find—

Bombay ... ... $y = 6{\cdot}1 \sin(\theta + 316^\circ)$.
Madras ... ... $y = 6{\cdot}2 \sin(\theta + 310^\circ)$.

Both equations give almost exactly the same range of the oscillation and nearly the same epochs of maximum and minimum as the sun-spots.[3] This result, which was wholly unexpected by me, is all the more remarkable that the two places are on the opposite coasts of India, and have their rains from different quarters. Calcutta, with a sufficiently large number of cycles, might also have agreed with Bombay and Madras, which is not the case, however, with four cycles only. In each case the criterion would show that no periodicity exists.

I cannot, then, agree with Gen. Strachey as to his test of periodicity nor to the conclusions he has deduced from it. I will not enter here into the consideration of the weights which may be given to results founded on the known principles of the calculus of probabilities, nor into the question whether the rainfall, not at one or two stations only, but over a country or the whole globe, may not show some relation to the sun-spot period as Mr. Meldrum believes, and as I think quite possible, judging from other results of solar actions. This relation, however, it appears to me has still to be proved, though the observations considered by Gen. Strachey are, on the whole, so much in its favour as to encourage further investigation. JOHN ALLAN BROUN

Lyndhurst, New Forest, July 18

In the paper read by Gen. Strachey before the Royal Society, May 24 (see NATURE, vol. xvi. p. 171), "On the alleged Correspondence of the Rainfall at Madras with the Sun-spot Period, and on the True Criterion of Periodicity in a Series of Variable Quantities," certain conclusions are arrived at which render it desirable to test the value of the criterion of periodicity employed. This is the more necessary when it is considered not merely that the principle, if a sound one, must be of

[1] The years given by the equation, the series commencing with 1813·5, are 1817·8, 1828·8, 1838·8, 1849·8, 1859·8, 1870·8. The condition that an oscillation should agree in its epochs of maximum and minimum with those of a known phenomenon (a very weighty one when the chances are to be considered) has been neglected by Gen. Strachey altogether.

[2] This refers to the periodic means deduced from the observed quantities; the above equation for Madras gives the observed means with a probable error of less than 3 inches.

[3] The first cycle at Bombay begins with the same year, (1824·5) as the second cycle for Madras.

extensive application in physical research, but also, and more immediately that the conclusion arrived at seriously affects the great modern problem of Indian administration, viz., the food-supply of the people. The conclusion is that in the case of the rainfall observations of Madras, which have been discussed by Dr. Hunter, the Director-General of Statistics to the Government of India, the evidence is not sufficient to establish either any periodicity or a correspondence such as Dr. Hunter points out. The correctness or incorrectness of Gen. Strachey's views will, it is evident, materially influence the line of action taken by the Government in dealing with the disastrous famines consequent on the recurring droughts of Southern India.

The criterion of periodicity brought forward by Gen. Strachey will appear from what follows. The mean rainfall at Madras for the sixty-four years is 48·51 inches; if we take the difference between this mean and the rainfall of each individual year, and average the results, we obtain 12·40 inches as the arithmetical mean of the sixty-four differences. If we now partition the sixty-four years' rainfall into six sun-spot cycles of eleven years each, and take the difference between each individual year's rainfall and the mean of that term of the sun-spot cycle in which that year's rainfall stands, and then average these sixty-four new differences, we obtain 11·20 inches as the arithmetical mean. Since 12·40 inches, the mean difference of the individual observations from the mean of the whole series, is, by the latter process, only reduced to 11·20 inches, or about 10 per cent., Gen. Strachey concludes that the supposed law of variation obtained from the means of the six eleven-year cycles hardly gives a closer approximation to the actual observations than is got by taking the simple arithmetical mean as the most probable value for any year; and that the evidence, therefore, is not sufficient to establish any periodicity in the rainfall of Madras, or any correspondence between it and sun-spots.

In illustration of his meaning Gen. Strachey applies his criterion of periodicity to the diurnal barometric oscillations at Madras, in which a well-ascertained periodicity exists. The result of the calculation is that the mean difference of all the individual observations from the mean of the whole series is 30, whereas the mean of the differences between the two-hourly individual observations and the averages of the same hours is reduced to 7, thus indicating, it is added, the distinct presence of a periodicity.

In concluding against the presence of any periodicity in the rainfall of Madras, Gen. Strachey makes these two assumptions:—(1) If there be a periodicity in the rainfall of this part of India, it would be made apparent by applying his criterion of periodicity to the observations; and (2) the difference between the two arithmetical means calculated as above must exceed at least 10 per cent.

The diurnal barometric oscillation at Madras is so regular a phenomenon that it was scarcely worth while to make the calculations, since one could have made a close approximation to the averages 30 and 7 by a simple inspection of the figures. Further, this periodicity which has been selected is altogether inappropriate to the subject in hand, as no one could possibly imagine for a moment that any periodicity which might characterise the rainfall of Madras would exhibit an approach to such regularity of occurrence as characterises the curve of the diurnal oscillation of the barometer at that place. The periodicities which suit the subject before us are such as are presented by the curves of the diurnal barometric oscillation in the British Islands.

Let us then apply Gen. Strachey's true criterion of periodicity in the examination of such a curve, taking for our example the barometric observations at Valencia for the month of December, 1876, as published in the *Hourly Readings* at their seven observatories, by the Meteorological Committee for that month. The examination will at the same time test the value of this new criterion as an implement of scientific research.

The mean of the whole 744 observations is 29·256 inches. The differences of the individual 744 observations from 29·256 inches give an arithmetical mean difference of 0·3372 inch: and the differences between each of the individual 744 observations and the means of their respective hours give an arithmetical mean difference of 0·3369 inch. Looking at the individual hours the largest differences are 0·3434 inch and 0·3413 inch at 10 P.M., and 0·3444 inch and 0·3459 inch at 6 A.M. Thus the difference of these two arithmetic means is only 0·0003 inch, and the largest difference for any hour amounts only to 0·0021 inch.

Now Gen. Strachey concluded against the presence of a periodicity in the rainfall of Madras on the ground that the two arithmetical means differed only 10 per cent. from each other. It follows *à fortiori*, if this new criterion is of any value, that the presence of a periodicity in the diurnal barometric oscillation at Valencia during December last be concluded against, inasmuch as the difference between the two arithmetical means is only $\frac{1}{10}$ per cent., and not rising quite to 1 per cent. for any of the twenty-four hours.

The averages for the month, however, show in an unmistakable manner the presence of such a periodicity. The mean pressure there fell to 29·245 inches the morning minimum at 7 A.M., rose to 29·263 inches the morning maximum at 11 A.M., fell to 29·238 inches the afternoon minimum at 3 P.M., and rose to 29·273 inches the afternoon maximum at 10 P.M., the time of occurrence of these phases of the curve differing no more than an hour from the mean periods for Valencia at this season of the year. The periodicity is, as stated, a well-marked one, the sum of the diurnal oscillations amounting to 0·106 inch, being three-fifths of the sum of these oscillations for Madras which average for December 0·174 inch. We do not require to remind our readers that the phenomena of the diurnal oscillations of the barometer take their place among the most universally accepted and best established periodicities of science.

In the concluding paragraph of his paper Gen. Strachey apparently applies another criterion to the Madras rainfall, which consists in the comparison of the successive combination of the observations, beginning with one eleven-year cycle, and then combining two cycles, and so on, till the whole six cycles were united. The result arrived at by this treatment of the figures, is that the successive means of the differences between the mean rainfall for the combined cycles and the mean for the several years of the cycle when combined, show no appreciable periodicity.

With reference to this mode of testing the question, it may be enough to say that even were the result of such an examination as adverse as possible, it could not be used as a conclusive argument against the existence of periodicity, for the very plain reason that we are investigating a periodicity for which only six terms or separate cycles are available, and in these cycles the non-periodical elements bulk largely. But the following table, referring to the portion of the diurnal barometric curve between the morning and evening minima at Valencia for December last will illustrate the point:—

	A.M.						P.M.		
	7	8	9	10	11	noon	1	2	3
Means for month ...	inches. 29·245	in. ·247	in. ·252	in. ·259	in. ·263	in. ·253	in. ·246	in. ·241	in. ·238
Days of month 3 ...	28·511	·533	·550	·561	·562	·555	·546	·535	·516
3–4 ...	28·490	·507	·527	·541	·551	·552	·554	·556	·550
3–5 ...	28·491	·502	·514	·526	·534	·535	·537	·541	·544
3–6 ...	28·589	·600	·613	·627	·637	·640	·644	·647	·652
3–7 ...	28·694	·707	·723	·738	·748	·751	·756	·760	·767
3–8 ...	28·863	·880	·899	·918	·931	·935	·942	·947	·957

The observations of the third will be seen to follow, though somewhat roughly, this portion of the curve for the month. The means of the third and fourth show a greater divergence from the monthly curve, and so on through the combinations, each successive combination showing instead of a continued approximation to, a continually increasing divergence from, the mean hourly variation of the month. It is this consideration, to which Gen. Strachey does not appear to have given due weight in his paper, which has led Meldrum and others in their investigation of periodicities of the rainfall and temperature, to extend their inquiries not only over lengthened intervals of time, but also over as wide areas as possible.

It may be added, that this new criterion of a periodicity enunciated and applied by Gen. Strachey at a meeting of the Royal Society in May last would, were it accepted, equally sweep from our view scores of periodicities now everywhere accepted, and effectually foreclose inquiry in many fields of research in which science is certain to reap brilliant results, namely, in those departments of research in which the non-periodical are very largely in excess of the periodical variations, of which meteorology may be regarded as presenting the most numerous and best illustrations.

GEOLOGICAL NOTES

German Geological Surveys—(1) Austria.—The programme of the Austrian Geological Survey for this year shows that the work is advancing, as it has been doing for some time past, mainly in two directions, one lying on the extreme east, the other on the far west of the empire. In the Tyrol two sections or parties are in the field; one of these, under Dr. Stache, and Mr. F. Teller, is investigating the crystalline masses of the Central Alps along both sides of the Vintschgau; the other, under Dr. E. v. Mojsisovics and Messrs. M. Vacek and A. Bittner, is engaged among the sedimentary formations between Botsen and the Venetian frontier. On the other side of the empire, in Eastern Galicia, Bergrath C. M. Paul and Messrs. Tietze and Lenz are busy among the Carpathians and their spurs to the south of Stanislawow. The vice-director of the Survey, Bergrath D. Stur, will also this year publish his researches on the flora of the Carboniferous period. The whole of the operations of the Survey are controlled and directed by the able hands of Ritter von Hauer.

(2) Province of Prussia.—Besides the national survey organised and paid by the government for the investigation of the geological structure of the kingdom of Prussia, there is in progress under the auspices of the Physical-Economical Society of Königsberg a geological investigation of the Province of Prussia with the publication of a map on the scale of $\frac{1}{100000}$. It might seem at a first glance that this wide alluvial plain could hardly offer much opportunity for geological observations or for much variety of colouring on the map. But by means of careful examination of the surface and well-arranged borings below it, much valuable information is being obtained regarding the structure and history of the alluvial, peaty, and drift deposits of the Baltic plain of Prussia. The lively interest which has been raised on all sides by the undertaking has suggested the idea to publish yearly an account of the progress of the work with notices of the more interesting observations and discoveries, and such additional information from other investigators or from other countries as may throw light upon the geological history of the province. Dr. Alfred Jentsch has prepared the first *Jahresbericht*, which appears in the *Transactions* of the Physical-Economical Society. After a brief account of the preparation of the map, and of the various boring operations he gives an interesting *résumé* of the geology and physical geography of East Prussia, including the variations of water-level in the Vistula and Pregel, the peat-mosses, marls, alluvial clays, drifts, brown-coal, and amber-deposits, with the cretaceous, Jurassic, and palæozoic rocks made known by boring explorations.

Geological Survey of Newfoundland.—Mr. Murray has published a second edition of the Index Geological Map of Newfoundland on the scale of twenty-five miles to an inch. It is of course brought up to date, and exhibits with great clearness the distribution of the various rocks of the colony. The remarkable serpentines, slates, and metamorphic rocks overlying the sandstones of the Quebec group on the west side of the island, are so inserted as to show distinctly their unconformable relations to the rocks below them. Four sections are likewise placed upon the map for the explanation of the geological structure of different regions. The map, in regard to execution, is all that could be desired, considering its small scale and provisional character. Mr. Murray's Report for 1876 has just been issued. The ice which hugged the coasts so late last year prevented a start being made until the end of June. During the few months available for exploration, Mr. Murray and Mr. Howley succeeded in mapping some portions of the interior about the Gander and Gambo rivers. As usual the routes lay along the river-courses where almost the only geological observations can be made, the intervening country being covered with swamps or forests. The Report shows that considerable areas of good agricultural land lie in the interior, and that while large masses of valuable timber exist they need to be guarded against the ignorant and wanton operations of lumber-men.

CHEMICAL NOTES

New Chromium and Manganese Compounds.—Some new compounds of chromium and manganese have lately been prepared and examined by Mr. J. B. Hannay, who has communicated a paper on the subject to the Glasgow Philosophical Society. On examining any general list of carbon compounds it is to be remarked that, however complicated their structure, they are not as a rule decomposed by water; on the other hand complex compounds of other elements are as a rule decomposed by this substance into two or more simpler compounds. Mr. Hannay was therefore induced to examine whether carbon is or is not the only element capable of forming series of bodies of complicated structure; and whether the existence of water on this earth is the reason of our not having complex bodies with other elements than carbon for their basis. The plan adopted was to take some complicated substance (containing no carbon) which is decomposed by water, find a solvent for it, and act on it with other reagents out of contact of air and moisture. The substance used was oxychlorid of chromium, (CrO_2Cl_2), and the solvents employed, carbon disulphide and carbon tetrachloride. Mr. Hannay has devised an apparatus which allows of the substance being precipitated, filtered, washed, dried, and weighed off for analysis without coming in contact with air or moisture. The following is a list of the chromium compounds prepared by him :—

$Cr_2Cl_4SO_3$.	$Cr_4Cl_6S_3$.	$Cr_3Cl_6P_2Br_6O_2$.
$Cr_2Cl_4SO_2$.	$Cr_4Cl_6S_3O_4$.	$Cr_3Cl_6P_46(H_2O)$.
$Cr_2Cl_4Br_2$.	$Cr_4Cl_6S_3O_{12}$.	$Cr_3Cl_6P_4$.
$Cr_2Cl_4S_2O_2$.	$CrCl_2S_2.H_2O$.	Cr_3Cl_6PCl.
Cr_2Cl_6S.	$CrCl_2S_2$.	Cr_3Cl_6PBr.
$Cr_2Cl_4Br_2S$.	$CrCl_6PSO$.	$(Cr_3Cl_6P)_2O$.
$Cr_2Cl_4S_2$.	$Cr_3Cl_6P_4O_6$.	Cr_3Cl_6P.
	$Cr_3Cl_6P_4O_2$.	

Mr. Hannay has prepared some analogous manganese compounds, but the analyses of these have not as yet been finished.

Complex Inorganic Acids.—Dr. W. Gibbs has lately obtained a series of new inorganic acids formed

on the type of the silico-tungstic acids obtained by Marignac. The new series of salts contain platinum instead of silicon, and the salt $10WO_3PtO_24Na_2O + 25 H_2O$ has been obtained by boiling platinic hydrate $Pt(OH)_4$ with acid sodic tungstate. Two metameric sodium salts have been obtained, one of an olive-green colour, the other honey yellow with an adamantine lustre. The corresponding potassium and ammonium salts of this platino-tungstic acid have also been obtained, but they belong to the yellow series. Mr. Gibbs has not as yet obtained salts corresponding to Marignac's twelve atom silico-tungstates. Acid molybdate of sodium also dissolves $Pt(OH)_4$, giving a green solution, which appears red when viewed in thick layers; the only salt of this series studied, crystallises in amber tabular plates having the composition $10MO_3PtO_24Na_2O + 25H_2O$. He is endeavouring to generalise the results by substituting other hydrates, such as $Zn(OH)_4$, $Ti(OH)_4$, $Sn(OH)_4$, but has, as yet, in these cases not obtained very definite results. He is also engaged in examining the phospho-tungstic acids containing $20WO_3$ obtained some time ago by Scheibler.

A SUPPOSED NEW METAL "DAVIUM."—The discovery of this new element is reported from St. Petersburg by Serjius Kern. It was found by him in the residues of platinum ores after treatment to separate out the metals of the platinum group. The specific gravity of the metal is given as 9·385 at 25°. The author supposes this new metal to occupy an intermediate position between molybdenum and ruthenium, but very strong evidence will be necessary to confirm the existence of a new metal belonging to the platinum group.

EFFECT OF PRESSURE ON CHEMICAL ACTION.—M. Berthelot, in a recent number of the *Bull. Soc. Chem.*, calls attention to the fact that some experiments lately made by Quincke have confirmed a statement made by the former chemist some time ago, that the evolution of hydrogen from zinc and sulphuric acid is not arrested by pressure. The experiments of Quincke show that when these bodies are brought in contact, the pressure of the hydrogen evolved rose in a few days from 1·5 to 10 atmospheres, and in a very much longer time from 25 to 126 atmospheres. Berthelot thinks that these experiments, although not performed for this purpose, prove that chemism is not modified, but only the nature and extent of the surfaces attacked. The evolution of gas would thus go on indefinitely, not arrested, but only modified in rapidity.

AMOUNT OF OXYGEN CONTAINED IN SEA-WATER AT DIFFERENT DEPTHS.—At a recent meeting of the Royal Society of Edinburgh Mr. J. Y. Buchanan communicated some results obtained from his experiments on the above subject during the cruise of the *Challenger*. Mr. Buchanan finds that at the surface the amount of oxygen varies between 33 and 35 per cent., the higher numbers having been observed in a water collected almost on the Antarctic circle; the smallest percentages have been observed in the trade-wind districts. In bottom waters the absolute amount is greatest in Antarctic regions, diminishing generally towards the north. The oxygen percentage is greatest over "diatomaceous oozes," and least over red clays containing peroxide of manganese; over "blue muds" it is greater than over "globigerina oozes." In intermediate waters the remarkable fact was observed that the oxygen diminishes down to a depth of 300 fathoms, at which point it attains a minimum, after which the amount increases. The following figures show the nature of this phenomenon:—

Depth (fathoms)	0	25	50	100	200	300	400	800	Between 800 and the bottom.
Oxygen (O + N = 100)	33·7	33·4	32·3	30·2	33·4	11·4	15·5	22·6	23·5

It is evident from these figures that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion may be drawn that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths; for, at less depths, there is more opportunity of renewal of the oxygen by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion is borne out by the experiments of Mr. Murray with the tow-net at intermediate depths, which go to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending much below 100 fathoms. Below 400 fathoms life is sparingly met with.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF 1605, OCTOBER 12.—It is known that Clavius attributed the ring of light which he observed round the moon during the eclipse of April 9, 1567, about the time of greatest obscuration at Rome, to the circumstance of the sun's disc not being entirely covered by our satellite, a narrow rim of light thereby remaining visible. As Prof. Grant relates, in his "History of Physical Astronomy," Kepler maintained that the luminous ring seen by Clavius could not have been the margin of the solar disc, because he found by calculation that the moon was at her mean distance from the earth, when her apparent diameter exceeds that of the sun, even in perigee; and when a similar ring of light was remarked round the moon during the eclipse of February 25, 1598, and attributed to the same circumstance, Kepler again pointed out that such an explanation was inadmissible, the moon's apparent diameter, on this occasion also, exceeding that of the sun. These opinions were expressed by Kepler in his work "Ad Vitellionem Paralipomena," published in 1604, and Prof. Grant remarks that an eclipse in the following year strikingly confirmed them. This refers to the eclipse of October 12, 1605, observed at Naples, of which Kepler writes thus: (*De Stella Nova in pede Serpentarii*, p. 116) "the whole body of the sun was effectually covered for a short time. The surface of the moon appeared quite black, but around it there shone a brilliant light of a reddish hue, and uniform breadth, which occupied a considerable part of the heavens." We follow Prof. Grant's translation of this passage, which clearly proves that the eclipse was total for a brief interval at Naples.

As the eclipse of 1605 first confirmed the accuracy of Kepler's views, in opposition to those of Tycho Brahé, who disputed the possibility of a total eclipse of the sun, it may not be without interest to examine the circumstances of the phenomenon as it would be observed at Naples. For this purpose the same system of calculation adopted for other eclipses mentioned in this column, is followed. The elements are:—

G.M.T. of Conjunction in R.A. 1605, Oct. 12, at 0h. 31m. 44s.

	° ′ ″
R.A.	197 14 51·0
Moon's hourly motion in R.A. ...	35 37·1
Sun's " " " ...	2 19·1
Moon's declination	6 40 27·9 S.
Sun's "	7 31 32·5 S.
Moon's hourly motion in decl. ...	10 50·2 S.
Sun's " " " ...	0 56·4 S.
Moon's horizontal parallax	59 21·2
Sun's " "	8·9
Moon's true semi-diameter	16 10·4
Sun's " "	16 3·9

The eclipse would therefore be central with the sun on the meridian in long. 11° 18′ W. and lat. 52° 26′ N., and the following would also be points upon the central line:—

Long. 19° 9′ E., lat. 39° 32′; and long. 14° 23′ E., lat. 40° 48′.

Calculating directly for Naples we find :—

	h. m. s.	
Totality began October 12 at	2 18 18	Mean time at
,, ended ,, ,,	2 19 28	Naples.

The duration of the total eclipse was 1m. 10s., which is in satisfactory agreement with the words of Kepler. The sun was at an altitude of 31°.

THE BINARY STAR α CENTAURI.—As far as can be judged from a projection of the measures published to the present time, it appears probable that the nearest real approach of the components in this binary is already passed, but that they will continue to apparently close-in until the angle is somewhere about 110°, when their distance may have diminished to $1\frac{3}{4}''$. We can only continue to urge upon southern observers the great importance of frequent measures of this object for some years to come, with all the precision that the case will admit of, that a problem of the highest interest in celestial mechanics may be fully investigated.

MIRA CETI.—This variable star is now close upon the epoch of minimum, as calculated from Argelander's formula of sines, and observations so far are much fewer in number near this part of the light-curve than about the maximum. The gradual ascent to the next maximum may be favourably watched in the present year; the date by the formula is November 10, 1877.

D'ARREST'S COMET.—By M. Coggia's observation at Marseilles on the morning of the 10th inst., it appears that M. Leveau's ephemeris gives the position of the comet within about 3'. Subjoined are the calculated places for Paris noon, during the next period of absence of moonlight :—

	Right Ascension. h. m. s.	North Polar Distance. ° '	Distance from the Earth.
August 8	3 57 35	83 45·0	1·559
,, 10	4 1 12	83 53·2	1·555
,, 12	4 4 43	84 2·0	1·551
,, 14	4 8 7	84 11·4	1·547
,, 16	4 11 24	84 21·4	1·543
,, 18	4 14 35	84 32·0	1·538
,, 20	4 17 38	84 43·2	1·533
,, 22	4 20 34	84 54·9	1·529

This comet has not yet been observed under its most favourable situation with respect to the earth. When the perihelion passage occurs early in August, it may approach our globe within 0·3 of the earth's mean distance from the sun, but, so far, has not been seen within a distance of about 0·8. At the next return at the beginning of 1884, observations will probably be difficult, but in 1890, when the perihelion passage (as well as can be foreseen without the calculation of planetary perturbations) is likely to fall in the latter part of August or in September, the comet's track in the heavens will be a favourable one.

NOTES

THE annual meeting of the Institution of Mechanical Engineers opened on Tuesday at Bristol. Mr. T. Hawksley, C.E., in his opening address, said it was the duty of the government to adopt such timely measures as would secure to us the paths of the ocean for our food inwards and our manufactures outwards. He deprecated the building of enormous and unwieldy floating castles, and advocated the construction of a fleet of swift, light, well-engined ships, equally capable of sailing or steaming. He thought the extreme action of some of the working classes the cause of England's trades going abroad. There was a conversazione in the evening.

A REMARKABLE case relating to manufacture and transport of explosives has just been the subject of an inquiry before the Wreck Commissioner. The facts are briefly these :—The passenger sailing ship *Great Queensland* left London for Melbourne on the 5th of August last. After the 12th, when she was spoken at sea, she was never seen; but some wreckage from her was washed ashore the same month on the south coast of England. She had taken on board some thirty-four tons of gunpowder, including two tons of the "Patent Safety Blasting Powder" (a compound made in North Wales by treating wood pulp with acid, and stated to have five times the strength of ordinary gunpowder !). There was also a large quantity of detonators and percussion caps. The stowage seems not to have been up to the mark; still the Commissioner regards it as having been fairly safe, but for the danger of spontaneous ignition of the patent powder, to which the facts apparently point as the probable cause of the disaster. The evidence bearing on the manufacture of the compound is not a little surprising. In 1875, the manager in charge of the process was a Mr. Hunt, describing himself as "an engineer, but no chemist." The powder he turned out seems to have been dangerously impure, and some of it having come into the hands of a Government Inspector was found so bad that a regular visit was made to the Company's works. Eight samples were analysed and pronounced impure and dangerous. Mr. Hunt was displaced. His successor, a Mr. Thistleton, made an attempt, at the directors' request, to re-dip the powder left by Mr. Hunt; but the smoke became intolerable, and at 110 deg. the sides smouldered into fire and dirtied everything about, while the heat broke the windows and charred the woodwork. He accordingly suggested that the only way was to dip it in potash solution. The process of remaking was going on in the early months of last year, and it was a portion of this remade impure powder of Mr. Hunt which was shipped on the *Great Queensland*. A few days after she sailed news came of an explosion at the Patent Gunpowder Works, and Major Majendie, having examined a cartridge found on the works after this, wrote that "accident is hardly the term to apply" to what happened. The conclusion of the Wreck Commissioner, then, is that the same event happened at sea and caused the disappearance of the ship. The facts speak for themselves. The case is evidently one of gross mismanagement based on an ignorance which might be laughable, though not excusable, in people employed in mixing tea and coffee, but shameful in the direction of a company for making an explosive. Considering the scientific knowledge imperative in making and handling our modern explosives, the appointment of the one manager who was "no chemist," and of the other who was so good a chemist (from the Royal Polytechnic) as to proceed to *re-dip* Hunt's material *in order to make it stronger*, at the request of his directors, and was only warned off when this compound nearly blew him into the air, calls loudly for explanation. It is important that the whole responsibility involved in this disgraceful case be fully elucidated by further inquiry.

WE regret to announce the death of Prof. Adolph Erman, the well-known physicist, which occurred in Berlin, July 13th. He was born in Berlin, 1806, and after completing a broad range of scientific study, devoted himself to physics, following in the path of his father, who was then professor of that branch in the Berlin University. In 1828 he joined the Norwegian expedition sent out to Siberia to investigate the phenomena of terrestrial magnetism. His own researches were carried out far beyond the confined limits of the expedition, and after thoroughly examining the hitherto almost unknown volcanoes of Kamtschatka, he terminated his journey by completing the circuit of the world in a Russian frigate. The rich store of magnetic observations made during the entire tour were gathered together into a work of two volumes. In 1834 Erman was appointed Professor of Physics at Berlin, a post which he continued to occupy up to the time of his death. From 1841–1866, he edited the *Archiv für wissen-*

schaftliche Kunde von Russland, a periodical issued at the expense of the Russian Government and designed to keep the world at large informed of the progress of scientific research in Russia. His investigations, extending into nearly every branch of the natural sciences, appeared chiefly in *Poggendorff's Annalen* and the *Astronomische Nachrichten*. The most valuable are his researches on terrestrial magnetism. In connection with H. Petersen he calculated the constants for Gauss's theory of terrestrial magnetism, based on his own multitudinous observations. A most valuable contribution to Gauss's theory is also to be found in his work on the magnetic phenomena of the year 1829, which includes a complete study of secular changes based on all then made observations. An equally exhaustive work on the magnetic phenomena of 1860, was left uncompleted at his death. In 1874 Prof. Erman was elected a fellow of the Royal Society.

WITH the view of extending the rudimentary teaching of physiology and the laws of health in elementary schools, the National Health Society has placed at the disposal of the School Board for London, in addition to a sum of 100*l*. offered previously, a further amount of 25*l*. annually for four years, to be given in premiums to those teachers and children who pass the best examinations in these subjects.

SINCE the institution of the Morgue in Paris, unidentified bodies have, it is known, been exposed, unclad, on stone slabs. It has now been decided (we learn from the *Revue Scientifique*) that the dead shall be placed before the eyes of the public just as they have been found, with the proper exception of those who bear on any part of their body a mark which may facilitate recognition. It is anticipated that this measure will increase by a third the number of identifications at the Morgue.—A course of lectures on legal medicine at the Morgue will be commenced in November. This practice has been discontinued for the last fifteen years.

THE Helvetic Society of Natural Sciences is to hold its annual congress at Bex, canton de Vaud, on August 20, 21, and 22.

THE intention is known to have been long cherished to erect a monument in Stockholm to Linnæus, and a sum of 45,000 crowns has been collected for the purpose. There have been two proposals, and to carry out the smaller of these the sum just named would be sufficient. But since the conviction has of late gradually gained strength that the statue should be raised on the so-called "Flora's Bakke" (Flora's Hill), in the Hop Garden, a desire has also grown to realise the larger proposal, according to which Linnæus would appear surrounded by four allegorical figures representing the four sciences to which he devoted himself, viz., Botany, Zoology, Mineralogy, and Medicine. On the understanding that the commune will supply the necessary means for the pedestal and for erection of the monument, a sum of 30,000 crowns was still required to give effect to the larger scheme, and a subscription list has lately been started by thirty influential citizens of the Swedish capital with this object. These thirty have together subscribed 15,000 crowns, and it may be anticipated that the remaining 15,000 will ere long be forthcoming.

WE have recently received a large number of reports of local societies, several of them containing papers of more than local interest, but to which we can refer only in the briefest possible way. The Norfolk and Norwich Naturalists' Society is numerically and financially stronger than at any previous period. Among the twelve papers published in its *Transactions* is one by Prof. Newton, giving an interesting account of the naturalisation of the Edible Frog (*Rana esculenta*) in Norfolk. Mr. Randall Johnson contributes an approximate list of the extinct mammalia of Norfolk.—The *Annual Report* of the Manchester Scientific Students' Association speaks favourably of its position and prospects, as does also the *Report and Proceedings* of the Manchester Field Naturalists and Archæologists' Society in the case of that Society.—The Cardiff Naturalists' Society is a large one and its thick *Report and Transactions* for 1876 contains several good papers.—Other Reports or Proceedings received are from the Bath Natural History Society, the Miners' Association of Cornwall and Devon, the South London Microscopical and Natural History Club, the Croydon Microscopical Club, the East Kent Natural History Society, the Geological and Polytechnic Society of the West Riding of Yorkshire, the Torquay Natural History Society, the Brighton and Sussex Natural History Society, Quekett Microscopical Club, and the York School Natural History, Literary, and Polytechnic Society. Of one or two of the papers in the Natural History Transactions of Northumberland and Durham we hope soon to give a detailed notice.

THE heat conductivity of hardened caoutchouc has been recently determined by Prof. Stefan, of Vienna. With six plates of equal thickness a parallelepipedal vessel was formed, and arranged as an air thermometer. The apparatus having acquired the temperature of a regularly-tempered room, it was inserted quickly in a vessel of broken ice; the time of insertion, and the position of the mercury in the manometer immediately observed, and then the times noted at which the mercury reached particular heights. The thermometric conductivity was found about $0{\cdot}000928\frac{\text{cm}^2}{\text{sec.}}$. Taking the specific heat of unit mass of vulcanised caoutchouc $= 0{\cdot}23$, and the specific gravity $= 1{\cdot}22$, it follows that the thermal conductivity $= 0{\cdot}00026$.

THE American expedition round the world, recently organised by Mr. Woodruff, of Indianapolis, is to start in October, and continue two years. Among the naturalists that have been engaged are Prof. Burt G. Wilder, of Cornell University, Dr. W. G. Farlow, Prof. Jenney, of Michigan University, Prof. Sidney J. Smith, of Yale College, Prof. S. C. Russell, of the School of Mines, Columbia College, C. Hart Merriam, and Dr. J. H. Kidder, U.S.N. The number of students is limited to eighty. The whole expense to each student is $5,000.

AT the last session of the Berlin Photographic Society a report was presented on the extent of photography in Germany from which we glean some interesting statistics. In the German Empire there are 3,000 photographers, who require each on an average 3 lbs. of nitrate of silver annually. Austria possesses the same number of photographers, but the average annua consumption of nitrate of silver is but 1 lb. Germany manufactures 20,000 reams of albuminised paper annually, of which but 1,000 are for home use. 40,000,000 cartes-de-visite were prepared in Germany during 1876.

THE Italian Committee for the exploration of Africa held its first session in June at Turin, under the presidency of the Crown Prince. It was decided to co-operate vigorously with the International Committee. The Italian Committee will devote its energies at first to the establishment and maintenance of a station at Shoa, where the Marchese Antinori is at present, regarding this as one of the most desirable positions from which to send out expeditions into the interior of Africa.

THE Scottish Meteorological Society holds its half-yearly general meeting in Edinburgh to-day. The business includes reports by Mr. Buchan on the temperature of the sea at Peterhead, and on the relations of the herring fishery to meteorology, for the four years 1873-76.

THE first number of Prof. Hoppe-Seyler's new quarterly journal, the *Zeitschrift für physiologische Chemie* has appeared. It contains valuable papers on the process of urea-formation in animal bodies, and the influence of ammonia salts on this

(Salkouski).—On aromatic substances in the animal body, and determination of sulphuric acid in urine (Baumann).—On animal and plant albuminous bodies (Weyl).—On lactosuria (Hofmeister); and on the physiology of lactic acid (Spiro). The journal is a decided acquisition to scientific literature.

WE have received the programme of excursions of the Manchester Field Naturalists' and Archæologists' Society for July to October. The seven excursions arranged (for Saturdays) appear to be of a varied and interesting character.

THE additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from North Africa, presented by Mr. H. B. Benson; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mrs. Escott; a Burmeister's Cariama (*Chunga burmeisteri*), a Brazilian Stilt Plover (*Himantopus brasiliensis*) from Buenos Ayres, two Black Swans (*Cygnus atratus*) from Australia, two Piping Guans (*Pipile cumanensis*) from Bahia, an Urumutum Curassow (*Nothocrax urumutum*) from Brazil, deposited; a Wapiti Deer (*Cervus canadensis*), an Indian Muntjac (*Cervulus muntjac*) born in the Gardens.

ASTRONOMICAL SYMBOLISM OF THE EAST[1]

THE two stars which in the book of Job are connected with Orion, and to which the Indian Orion-legend referred, are connected with the two red stars or Rohini of Indian traditions. It is shown that these two fixed stars, observed as contemporaneously rising and setting on the horizon, formed the unchangeable starting points for regulating the lunations, and that they thus brought about in course of time an absolute correct chronology. The early discovery of equinoctial precession led to the substitution of these fixed stars by the changeable equinoctial points, till Copernicus, by separating the latter from the solar path, re-established the correct measurement of time by referring the solar motion to fixed stars. The determining single stars, later, constellations nearest to the equinoctical points, to which former, both Chinese and Persian, traditions refer, became the symbol of the order manifested by the heavenly bodies, which cosmical order was attributed to the Deity. The symbol of the two cherubs or kirubs, that is "bulls," in the language of cuneiform inscriptions, are shown to have referred to the rising and setting of the constellation of Taurus, which being called Kirub at its rising, was called Seraph, or Ser-Apis, literally, "the grave of the bull," at its setting. The Pleiades in the neck of Taurus stand in the same relation to this constellation as the god Sebaot, the god of the Sheba-ut, or seven stars, the Sibut of the Babylonians, to the Cherub. The symbol of the chariot of the Cherubim, and of Jehovah riding on the Cherub, as the Pleiades may be said to be riding on Taurus, are thus astronomically explained, and connected with the representations of Ormuzd riding on the winged bull, as also with similar Mithraic representations. The fortnightly period of the Hebrews, from the new moon to the full moon, in connection with the precessional cycle of seventy-two years, probably known to the Hebrews, is shown to have formed the basis of the Osiris-Typhon legend, which was fully developed before the commencement of Egyptian history. The fourteen divisions of the litanies of Thot, the god riding on the moon, and whose secret number was seventy-two, are explained by reference to fourteen moon-stations of the lunar zodiac, the hidden Mazzaroth or mansions of the moon in the book of Job, and to the precessional cycle of seventy-two years, with which can be connected the solar year and the Phœnix period of the Egyptians, the Saros of the Babylonians, and the Mosaic period of one day like a thousand years, as well as several other Babylonian periods; also the number of the sons of Japhet, and the genealogies in Luke from Seth to Joseph, the husband of Mary. When the solar zodiac had taken the place of the lunar zodiac, when the two determining fixed stars had been replaced by the changeable equinoctial points, and the commencement of spring and of autumn became the fundamental symbol of all religions, the ideal heroes of light were connected with the spring-equinox, at first in Taurus, and the ideal heroes of darkness with the autumn-equinox, at first in Scorpio, close to which is the constellation of the Serpent. Thus to Ormuzd, Indra, Osiris, Dionysos and Apollos, were respectively opposed the serpent deities Ahriman, Ahi, Typhon, the Titans and Python. So also the Messiah, "The Sun of Righteousness," and "the day-spring from on high," was opposed by Satan, literally the antagonist, "the old serpent" the devil. The transition from the sacrifice of bulls to the sacrifice of lambs, and the vicarious and sin-removing character of these sacrifices in pre-Abrahamic times, is shown to have been connected with, and probably to have been caused by equinoctial precession, by Aries having taken the place of Taurus. Some of the mysteries of the Great Pyramid are explained by Eastern astronomical symbolism and the two passages pointing north and south are shown probably to have referred to the approximatively contemporaneous midnightly culminations of Aldebaran and Antares at the autumn equinox, as observable at places in the latitude of the Great Pyramid, and likewise in the latitudes of Bactria and Northern India, during a period of about 150 years, within which the year 3300 B.C. falls. The fact that Chinese, Indians, and Arabians, at a remote period, counted twenty-eight moon stations, but that there is nowhere a trace of twenty-nine mansions of the moon, is submitted as possibly implying a date for the earliest astronomical observations of the East transmitted to us, at a time when the lunar month, now having a duration of about twenty-nine and a half days have only twenty-eight days, or rather, not yet twenty-nine. As the mean motion of the moon is the same in long periods, this period of twenty-nine days would have probably commenced about 600,000 years ago, if the retardation of the earth's rotatory motion, by which alone the prolongation of the lunar month seems to be explainable, really does amount to twenty-two seconds in a century, as now asserted by high authorities.

[1] Abstract of paper read at the Society of Biblical Archæology, "On Astronomical Symbolism of the East, as transmitted by Hebrews and Christians," by M. Ernest de Bunsen.

THE NORWEGIAN EXPEDITION TO THE NORTH SEA

FROM a letter by Prof. G. O. Sars, in the *Christiania Dagbladet*, dated Bodö, June 24, we make the following quotations with reference to the progress of the Norwegian expedition which left Bergen on June 11:—

"On the 16th of June we had arrived sufficiently far northward to commence our labours, and sounding-lines, thermometers, dredges, and trawling-nets were at once called into use. Since then the work has been pursued unremittingly, despite stormy weather, and we have every reason to be satisfied with the results so far. The hitherto-unknown contour of the sea's bottom between Folden fiord and the Lofoten Islands is now so clearly ascertained by means of our transverse section, that we can map out to the north with a certain degree of precision the curve of the extended barrier, which keeps back the cold water coming from the depths of the Polar Sea. We have found the curve somewhat different from our expectations, especially in the neighbourhood of the Lofoten. The soundings appear to indicate the presence of a remarkable indentation, similar to the one on the southern part of the coast, and we have found a precipitous slope of the sandbank resembling that of the well-known "Storeg" near Aalesund. It is evident that we have encountered here a most important submarine conformation. The consideration of its effect on our sea-fisheries will be delayed until more detailed surveys have been carried out. In the course of our soundings on the way to Bodö, we were able by means of the improved Negretti-Zambra thermometer to establish beyond the range of doubt the presence of a layer of warm water below a layer of cold water of considerable depth.

"Our zoological acquisitions have been highly satisfactory, especially those in the cold zone. We have added several species to the list of the previous expedition; amongst them some hitherto detected in the Polar Sea only and others entirely new.

"The voyage will be pursued to Röst, where several days will be spent in magnetic observations, and in gathering zoological specimens. The latter promise to be of value on account of the zoographical interest of the locality, which has as yet been left unvisited. The section from Röst will be followed carefully in order to determine with certainty the expected bend in the sandbank. The progress of our expedition shows us more and more the fundamental importance of an accurate knowledge of the physical nature of the North Sea, not only for Norway, but also for the solution of the general questions with regard to the physical and biological conditions of the ocean in general."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

BERLIN.—No small degree of excitement has been caused in the university circles of Germany by the expulsion of a Privat-docent, Dr. Dühring, from the University of Berlin, not only on account of the great rarity of the occurrence, but on account of the circumstances leading to the act. Dr. Dühring, who has suffered from blindness since the commencement of his pedagogic duties in 1865, has developed, notwithstanding, a remarkable degree of mental activity in a variety of directions, issuing works on political economy, metaphysics, mathematical physics, &c., and attracting a numerous auditory by the radical and unfettered character of his utterances on these subjects. His unsparing denunciation of the more shadowy side of German universities, the system practically in vogue with regard to the appointment of professors, &c., has induced a somewhat wide-spread feeling of bitterness towards him, which has led to an accusation before the university based on the unmeasured personal criticism of his colleagues, especially Prof. Helmholtz, in lately issued works. The expulsion has led to the holding of meetings and issuing of addresses by the more radical portion of the students, who regard the section of the Prussian constitution, "die Wissenschaft und ihre Lehre ist frei" as endangered; and the whole affair has led to a thorough ventilation of the condition of the universities in the press of the empire.

RUSSIA.—The Russian Government expends 2,140,000 roubles (over 300,000*l.*) on its seven universities, of which sum the Moscow University receives the most—425,000 roubles.

SALARIES OF GERMAN PROFESSORS.—From a recent statement with regard to the salaries in the Berlin University, we notice that the two well-known professors of chemistry and physics receive each 1,500*l.* independent of the lecture receipts, while 900*l.* is the salary of a number of other leading professors. These figures are, however, much above the corresponding ones of other German universities.

STRASSBURG.—According to an imperial order the University receives in the future the title "Kaiser Wilhelms Universität." The present attendance—658—shows for the first time since the re-establishment of the University a decrease in the number of students. The faculty embraces 106 professors and privat-docenten. The large anatomical laboratory is approaching completion.

TÜBINGEN.—The present number of students in attendance on the University amounts to 1,103, the largest number attained since its foundation. Extensive preparations are being made for the celebration in August of the 400th year of its existence.

ZURICH.—The University has at present an attendance of 324, of which the majority are in the medical faculty. Of the seventeen female students fourteen study medicine and three philosophy.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Microscopic Science* for July contains several articles of special interest. First is the fourth part of Mr. Archer's *résumé* of recent contributions to our knowledge of "Fresh-water Rhizopoda," in which *Lecythium hyalinum*, *Chlamydophrys stercorea*, *Platoum parvum*, *Gromia paludosa*, and *Cyphoderia truncata* are described and figured.—Following is an abstract of Mr. E. C. Baber's researches on the lymphatics and parenchyma of the thyroid gland of the dog, with illustrations, published at length in the *Philosophical Transactions*.—Dr. Watney also, from the same *Transactions*, gives, with a plate, an account of his study of the minute anatomy of the alimentary canal.—Dr. Angelo Andres describes and figures a new genus and species of *Zoanthina Malacodermata* (*Panceria spongiosa*).—Prof. Franz Boll follows with his "Contributions to the Physiology of Vision, and of the Sensation of Colour."—Mr. A. Sangster has observations on the muscular coat of sweat glands, in which it is shown that the epithelium of the gland rests directly upon the muscle surrounding it.—Dr. Klein contributes a paper on the minute anatomy of the omentum, describing certain bud-like structures occurring on the fenestrated portion, the method of production of the fenestræ, and some points in the formation of the blood-vessels.—Mr. F. Darwin writes on the protrusion of protoplasmic filaments from the glandular hairs on the leaves of the Common Teasel, in which he helps to develop the important principles of the incipient steps in the formation of special organs.—Mr. H. N. Moseley gives notes on the structure of several forms of Land Planarians, with a description of two new genera and several new species, and a list of all species at present known.—Notes and memoranda conclude the number.

Bulletin of the Buffalo (N.Y.) *Society of Natural Sciences*, 1877. This society has published a valuable list of the freshwater fishes of North America by Mr. D. S. Jordan. Mr. M. C. Cooke is contributing a similarly exhaustive list, with references, of the hyphomycetous fungi of the United States.—An interesting paper by Mr. F. S. Dellenbaugh gives some account of the so-called Puebla Indians of the Rocky Mountain region, also known as Shinumos. They were not really of Indian race; they seemed to have died out in consequence of the incursions of the true Indians. The name Moquis, by which seven of their villages are designated, signifies "the dying race." In their retreat before the enemy they occupied the most inaccessible retreats in the cañons, built cliff houses, cultivated minute patches on the cliffs, lived in caves, &c. They appear to have had considerable artistic skill, from the designs, and even paintings, left on sandstone in the walls of houses, &c. The writer urges a careful exploration of all the extant remains of these people, for they are rapidly being destroyed by careless and ignorant settlers.

SOCIETIES AND ACADEMIES

LONDON

Meteorological Society, June 20.—Mr. H. S. Eaton, M.A., president, in the chair.—Henry Hearder and Henry Law, M.Inst.C.E., were elected fellows of the Society.—The following papers were read:—On an improvement in the mechanism of self-recording meteorological instruments, by the Rev. C. J. Taylor. In order to obviate sluggishness in self-recording aneroids and ordinary small-bore mercurial barometers, the author places a small electric bell apparatus, from which the bell has been removed, so that the clapper when in action shall strike on the top of the vertical brass bar on which the recording pencil slides; by this means a very rapid succession of light taps can be administered at a point which affects all the moveable parts of the mechanism.—Results derived from the sunshine records obtained at the Royal Observatory, Greenwich, by means of Campbell's self-registering sun-dial, during the year ending April, 1877, by W. Ellis, F.R.A.S. The instrument consists of a very accurately-formed sphere of glass four inches in diameter, supported concentrically within a well-turned hemispherical metal bowl in such a manner that the image of the sun formed when the sun shines falls always on the concave surface of the bowl. A strip of some material being fixed in the bowl, the sun, when shining, burns away the material at the points at which the image successively falls, by which means a record of periods of sunshine is obtained. The duration of sunshine in hours for each month of the year ending April 30 (excepting from May 1 to 6, July 31, and October 27 to 31), was as follows:—

	hrs.		hrs.		hrs.		hrs.
May	152·3	Aug.	216·9	Nov.	35·9	Feb.	36·4
June	184·5	Sept.	106·1	Dec.	6·5	Mar.	99·3
July	214·3	Oct.	47·3	Jan.	18·7	Apr.	71·8

The greatest daily duration was 13·9 hrs. on June 11, daily durations exceeding 10 hrs. occurred six times in May, eight in June, ten in July, and eleven in August. One of the most remarkable periods was August 7 to 14, the duration having exceeded ten hours on every day during this time. It appears that in the months of August, September, and October, the mean maximum and minimum temperatures were both, on the average, higher on days of greater sunshine than on days of lesser sunshine, whilst in the months of January and February, an exactly opposite condition existed. In the remaining months the mean maximum temperature of the greater sunshine group was higher, and its mean minimum lower, than the corresponding mean maximum and minimum temperatures of the lesser sunshine group. It also appears that there was more sunshine after noon than before noon in every month, except August, March, and April.—On the diurnal variation of the barometer at the Royal Observatory, Greenwich, by W. Ellis, F.R.A.S. The Astronomer-Royal having communicated the numerical values of the variations of the barometer as deduced from the photographic records at the Royal Observatory, during the twenty years ending

1873, the author adds a few general remarks thereon. Comparing together the different months of the year, it is observed that the morning minimum and the forenoon maximum both occur earlier as the year advances; they are earliest in summer, and become later again on the approach of winter. The afternoon minimum and evening maximum, on the contrary, occur later as the year advances; are latest in summer, and become earlier again towards the end of the year. They all change in a certain degree with the changes in the times of sunrise and sunset. As a consequence of this the intervals between the morning minimum and the forenoon maximum, and between the afternoon minimum and evening maximum do not change very much through the year, whilst that between the forenoon maximum and afternoon minimum is much shorter in winter than in summer, and that between the evening maximum and morning minimum is much longer in winter than in summer.—On the rainfall of Jamaica during the seven years 1870-76, by Griffith N. Cox.—Contributions to the meteorology of Cannes, by William Marcet, F.R.S.—Mr. Marriott exhibited and described Bogen's hygrometer and new standard siphon barometer. The barometer possesses the following special features:—It is so constructed that it can easily be put together and taken to pieces again. The long leg consists of a tube of the same diameter throughout, and is supplied with a glass stopper which has a very fine bore passing through its centre, enabling one to fill the tube with mercury in one or two minutes and to completely exhaust it of air in one or two minutes more. The open end of the long leg is ground air-tight to the short (curved) leg and can be instantaneously put together or separated. The long tube alone is graduated so as to have only one scale, but by a peculiarly-constructed screw of given length placed on the short tube a precise and accurate reading can be easily obtained. The barometer is mounted on a peculiarly-shaped stand which has three adjusting screws by means of which the perpendicularity of the instrument can be ensured.

Paris

Academy of Sciences, July 16.—M. Peligot in the chair.—The following papers were read:—Anthrax and septicæmia, by MM. Pasteur and Joubert. Anthrax may be called the disease of (what the authors call) *bacteridie*, as trichinosis is the disease of the trichina. The blood of a healthy animal contains neither microscopic organisms nor their germs. That of an animal having anthrax contains no organisms but the *bacteridie*, which is aerobic, so that the anthracic blood is imputrescible of itself. In the carcase the blood putrefies through vibrios lodging in it. The *bacteridie* disappears in liquid, in presence of CO_2. It is a mistake that putrefaction, as such, destroys the anthracic virulence. The development of the *bacteridie* takes place difficultly when in the presence of other organisms. These observations the authors apply to certain facts of practical experience. They state that septicæmia is produced by a *vibrionien* as anthrax is by a *bacteridie*.—Experiments, according to which the fragmentary form of meteoric irons may be attributed to a rupture under the action of strongly compressed gases, such as arise from explosion of dynamite, by M. Daubrée. To steel prisms were attached charges of dynamite, which were exploded in clay pits, so that the parts could be brought together after rupture. The alveoli produced (often grouped) with projecting rim, were of more pronounced character than those from powder. The surface was sometimes raised in long bulging ridges. The surfaces of fracture were some of them polished, others striated.—Researches on the tertiary strata of Southern Europe, by M. Hebert. This first paper relates to the eocene and lower miocene of Hungary.—M. Dumas, referring to a recent paper by M. Sée, affirmed that the real inventor of salicylic acid was the eminent Italian chemist M. Piria.—On a disease of the grape in the vineyards of Narbonne (June and July, 1877), by M. Garcin.—Observations of D'Arrest's periodic comet at the Marseilles Observatory, by M. Stephan.—Note on the theory of quadratic forms with any number of variables, by M. Frobenius.—Demonstration of two geometric laws enunciated by M. Chasles, by M. Fouret.—On the division of the circumference into equal parts, by M. Lucas.—Researches on the compressibility of liquids (continued), by M. Amagat. *Inter alia*, the coefficient of compressibility increases when the pressure increases for all liquids in which it increases with the temperature. The compressibility of successive terms of the family of formenic carburets decreases regularly as we descend in the series, both at 100° and at ordinary temperature. The presence of sulphur, chlorine, or bromine, in liquid bodies, tends to render them less compressible.—On the electric and capillary properties of mercury in contact with different aqueous solutions, by M. Lippmann. When mercury is in contact with pure or acidulated water, the addition of a small quantity of certain substances to the water changes notably the capillary constant and the electromotive force. M. Lippmann proves that for each value of the latter the capillary constant has one, and only one, determinate value independent of the chemical composition of the liquid. That is, if for two different combinations the electromotive force is the same, the capillary constant is also the same.—On the vapours of alcoholate of chloral, by M. Troost.—Action of light on hydriodic acid, by M. Lemoine. Hydrogen and iodine do not sensibly combine in the cold state under the influence of light. It may be inferred that the decomposition of hydriodic acid by the sun is unlimited. The slow decomposition in light might be used to measure the degree of illumination of the sky in large meteorological observatories. In a month of insolation in the cold state 0·80 of gaseous hydriodic acid is decomposed, whereas heating one month at 265° decomposes only 0·02, and even at 440° during a few hours, only 0·20.—Note on a new derivative of indigotine, by M. Schutzenberger.—On the properties of resorcine; thermo-chemical studies, by M. Calderon.—On the reform of some processes of analysis used in the laboratories of agricultural stations and observatories of chemical meteorology; second memoir, acidimetry, by M. Houzeau.—On the nature of acids contained in the gastric juice (continued), by M. Richet. There is in gastric juice an organic acid soluble in ether, and it is probably sarcolactic acid.—Note on numeration of blood corpuscles in diphtheritis, by MM. Bouchut and Dubrisay. In this disease there is a considerable increase in the number of white corpuscles, and a diminution of that of the red.—On the influence of excitations of organs of sense on the heart and the vessels, by MM. Couty and Charpentier. Such phenomena seem to be produced, not by the sensorial perception itself but by an ulterior cerebral work which might be called *emotional*.—Experiments proving that neither pure air nor oxygen destroy the septicity of putrefied blood, by M. Feltz.—Researches on a case of congenital ectopia of the heart, by M. François-Franck.

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